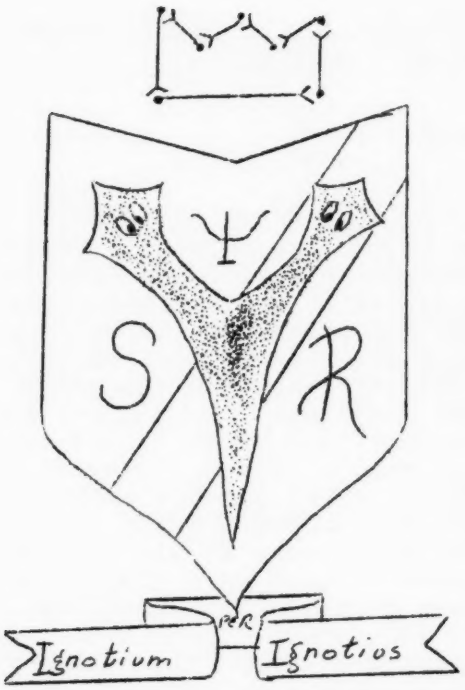

THE WORM RUNNER'S DIGEST

PLANARIA

My habit strength
Is my body length.
My diameter, D,
Equals energy.
Pi D times length
Yields behavior strength.

Causally or randomly
Thoughts regrow rapidly.
My anterior
Is not superior
Nor my posterior
At all inferior.

Basal, dorsal, rostral, ventral;
Though I know my nerves are central
I have made full provision
That, if I suffer long division
Mem'ry will spread equally
In all my parts or sequellae.

—John W. Cotton

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August, 1961

THE WORM RUNNER'S DIGEST
An Informal Journal of Comparative Psychology
Published Irregularly By
The Planaria Research Group

Department of Psychology
The University of Michigan
Ann Arbor, Michigan

Britannica Center
Box 11576, Station A
Palo Alto, California

August, 1961 THE WORM RUNNER'S DIGEST Vol. III, No.2
An Informal Journal of Comparative Psychology

WORMS AND THINGS

*When a flatworm gets too big, or too educated and worldly-wise, it has a tendency to split. What's true of planaria is also true of the Planaria Research Group, and this is the first official announcement that we're twinning. The Editor of this Journal (and nominal Chief Ganglion of the Group) has taken a year's leave of absence from Michigan to accept the position of Associate Director of the newly created Britannica Center for Studies in Learning and Motivation, at Palo Alto, California. As part of the "lure" to get the Planaria Research Group, ALLEN CALVIN, Director of the new Britannica Center, offered us one of the most magnificently equipped laboratories in the country, so we are guaranteed a fine new home. REEVA JACOBSON and ALLAN JACOBSON (remember that these two are not kin to each other) will join the Editor at the new Center, and DANIEL KIMBLE, a more-or-less ex-officio member of the Group these days, will be on a post-doctoral fellowship at Stanford at the same time. Meanwhile, the Michigan section of the Group will remain under the directorship of MARGARET CLAY, and will include PAUL and GRETCHEN CORNWELL and RAY KAUFFMAN, as well as HARRY DOERR and JIM LANGHOFF. We have hopes that twice as many laboratories will mean twice as much research.

And, where the Editor goes, there algo goes the DIGEST. So, beginning with the next issue, the DIGEST will be issued under the imprimatur of Encyclopaedia Britannica Films (and a low bow to EBF for picking up the tab). All correspondence should, after September 1st, 1961, be addressed to:

WORM RUNNER'S DIGEST
Britannica Center
P.O. Box 11576, Station A
Palo Alto, California

*Thanks to the magnificent description of our work by ARTHUR KOESTLER in the April 30th issue of the LONDON OBSERVER, reprinted in DIE ZEIT, in a Toronto newspaper and elsewhere, the DIGEST is now more international than ever. Just the week this paragraph was written, for example, we received subscription requests from Dublin, from London, from Buenos Aires and from New Zealand. Nothing could make us happier than adding new overseas readers to our mailing list. But that strange noise recently heard coming from the Editor was not a sound of job--rather, it was a muted and fleeting sigh of envy over KOESTLER's incredibly dexterous handling of the English language. One of the last Palinurian joys of this life is reading whatever KOESTLER writes. Once we had pretentions of being a writer ourselves, but we know when we're bested. KOESTLER's is perhaps the best account of our work in print anywhere--even the Planaria Research Group learned something about its own endeavors from reading KOESTLER's account. It would please our vanities immensely were we to reprint this article in

the DIGEST, but that does seem a little blatant on second thought. So, we'll take the better part of valor and say we've some dittoed copies of same on hand and any reader who missed the article in the OBSERVER can have his very own reprint by writing to us for same.

*Speaking of things to write about, we are most happy to announce that ALLAN JACOBSON's extensive survey of the literature on "Learning in Platyhelminthes and Annelids" has been accepted for publication by HARRY HELSON for the Psychological Bulletin, and should appear in print some time in 1962 or 1963. For those of you too eager to wait, we have a few mimeographed copies left for general distribution. After these are gone, you'll simply have to wait for the article to appear in the Bulletin. Write to ALLAN at the Britannica Center (see above address).

*Recent developments in the art of worm-running include the following:

(1). We hear that Professor ROY JOHN, of the University of Rochester, recently announced at a scientific meeting that he had repeated the "cannibalism" study we described in the last issue of the DIGEST, and that he got precisely the same results we did. That is, naive cannibal planaria were fed on their conditioned compatriots and then were trained to criterion. Their training was compared with that of cannibals which feasted on untrained worms. Sure enough, the cannibals which ate "educated" friends and relations reached criterion significantly more rapidly than did cannibals which devoured unschooled or truant planaria. Since JOHN's replication was apparently performed "blind"--that is, the experimenters did not know until the end of the study which animal was in which group--perhaps there's something to our "ingestive theory of transfer of training" after all. We're particularly gratified at this replication by ROY JOHN and his students since one of our esteemed British colleagues suggested in print that our study was "most doubtful" and criticized KOESTLER for daring to mention cannibals in his OBSERVER article. In this case, at least, our vindication was not long in coming.

(2). JAY BOYD BEST reports that he's having considerable success with the prototype of his "Best Box for Worms." JAY has seemingly trained planaria to poke their heads in between two narrowly-separated uprights. This action breaks a beam of light which registers on a photo-electric cell and turns off a noxious stimulus (at the nonce, a bright overhead light). Preliminary data suggest that worms may give much the same sort of "cumulative record" of responses as do rats, pigeons, psychotics and other strange beasts. We hope for a fuller account in a future issue of the DIGEST.

(3). A long time ago, back in 1957 when we had just completed our first study on retention following regeneration, we asked ourselves what else flatworms might be taught other than a conditioned response. "Nothing else," we told ourselves, and since we considered ourselves the world's greatest expert on flatworms, that was that. Luckily for science (if not so fortunate for our ego, ED ERNHART, JAY BOYD BEST and several others hadn't

the benefit of our wisdom and proceeded to teach their animacules various types of mazes. We bit our lips in rather stunned appreciation of these educational feats, but decided that we'd "stick to what was good enough for the pioneers" and kept grinding away on light-shock conditioning studies until we (and our worms) were blue in the face. We don't regret this--we learned a lot from our work. But of late we've loosened up a little and have begun to teach our worms some a-maze-ing tricks ourselves. You'll find BARBARA HUMPHRIES' account of this work elsewhere in the DIGEST. The important thing to note now is this:

It takes much patience, skill, and precisely the right sort of conditions to achieve clear-cut classical conditioning in planaria. Having been at it for 9 years, we've probably more successful in getting it than many other laboratories, but even we don't succeed all the time. There's always a nagging question of doubt in our minds, too, since the conditioned response often looks little different from the natural response many worms give to the onset of photic stimulation. There's no mistaking the fact that worms can be conditioned, but it often takes hundreds of trials and then the experiments succeed only if the experimenter is exceedingly careful and rigorous.

On the other hand, nothing could be simpler than running worms in the sort of T-maze that BARBARA described, as ED, JAY and many others have known all along. Please inspect the data BARBARA presents quite carefully. Worms can be trained to a most strict criterion in less than 100 trials; regenerated pieces usually show complete, or almost complete, retention of the maze habit. And one doesn't have to worry about shock intensity, light intensity, response intensity, or a variety of other contaminating factors.

We seriously recommend (admittedly, at quite a late date) that anyone reading the DIGEST who has attempted to condition worms, or is attempting to do so now, should consider switching over to mazes instead. The results are far less ambiguous, the training period shorter (in terms of the number of trials to criterion), the experiments far less dull to run. And the possibilities for new sorts of studies almost endless...

(4). A recent trip took the Editor to Brookhaven National Laboratory, to inspect the joint research on worms we're carrying on with SIMON FREED and HENRY QUASTLER of the Chemistry and the Biology departments, and with WILLIAM CORNING, who's working at BNL this summer as a fugitive from Rochester. We can report that the work cometh slowly, as does all research at the beginning of a project, but that it cometh exceedingly well. We are learning how to inject various radioactive tracer substances into the worms, and how much radioactivity the worms will tolerate before dying. Soon we should have some idea whether the "memory" in these beasts is carried in individual cells and, if so, what part of the cell actually contains the "memory."

*We don't usually go out on a limb and recommend books to our readers, unless they be the special and highly scientific tomes that everybody ought to read, or at least have on his shelf to impress callers. But two recent acquisitions to the Worm Library are so good they ought not only to be bought by everyone, but also they deserve being read.

First, LEO SZILARD sent us the paperback edition of his Simon and Schuster book, THE VOICE OF THE DOLPHINS. This noble work contains a number of fascinating, scintillating science fiction stories by one of America's greatest adopted scientists. This is science fiction, and not the pale pap often pawned off on the unwary reader by anti-science writers. There's a lot of comparative psychology to be found in the stories, too--so run, do not walk to your nearest bookstore. This will become a classic.

A book that's already a classic, that's also definitely a contribution to the field of comparative psychology, that's also science fiction, is KAREL CAPEK's viciously amusing, penetrating satire, THE WAR WITH THE NEWTS, now out in paperback by Bantam Books. If there is a more biting, more knowing dissection of the behavior of many "learned gentlemen and scientists," we've yet to discover it. We recommend particularly that section of the novel in which various zoologists dismiss the fact that the "newts" can be taught to speak English intelligently by announcing that, after all, the animals couldn't speak it better than most people could, that all the newts knew was what appeared in the evening papers (the only source materials then allowed the newts) and that they scarcely showed more intelligence than did the average Englishman. Ipso facto... Pompous people should be required to commit the book to memory.

(A puzzled aside: CAPEK, KOESTLER and SZILARD were all born within a couple of hundred miles of each other. Should us native-born, true-blue American scientist-writers complain to the government about a possible foreign monopoly, cartel or vertical brain-trust?)

*We have discovered two new CORNWELLian worm runners. To begin with, GRETCHEN and PAUL CORNWELL are the pleased parents of a girl-type human offspring whom they guarantee will not grow up to be a flatworm. And while we were at Brookhaven, we ran across ANNE CORNWELL (no kin), who took her doctorate at HEBB's lab at McGill and who's now running worms at Yeshiva. Planaria are obviously both CORNWELL- and JACOBSON-o-trophic (or vice versa).

*In the last issue, we failed to note that two of the studies presented therein, performed here at Michigan, were sponsored by grants from the Atomic Energy Commission and the National Institute of Mental Health. We hope our good sponsors will forgive this oversight. AEC and NIMH have been providing us with badly needed and most welcome research funds for the past two years and (let us hope) will continue to do so for the foreseeable future.

*In This Issue... The cover poem is by JOHN W. COTTON, a professor of psychology at the University of California (Santa Barbara). Would that all professors of psychology were blessed with COTTON's erudite wit... We wish we could tell you who SIGMUND FRAUD is, but are restricted to saying he's a most bright young psychologist now working in industry. Since most industrial companies require prior permission before their employees may publish, well, how would YOU explain a publication in something called the WORM RUNNER'S DIGEST to a Board of Trustees?.... TOM MORRILL's lovely ALICE is but the first instalment of (we hope) a continuing spoof of the neo-Darwinians and quasi-Lamarckians.... ROBERT SOMMER has deserted the hospital world and is now an assistant professor at the University of Alberta.... Typing and other cumbersome chores this issue by CONNIE BANNASCH and JOYCE PACE.... And our favorite cover girl REEVA JACOBSON has done it again....

*Good Reading!

James V. McConnell
Editor

TITLE: Alice's Adventures in Evolutionland: Part I

AUTHOR: Thomas Morrill
1403 Stone Road
Tallahassee, Florida

I don't like controversy, yet it seems that whenever my nine year old girl child and I are together, we become embroiled in religion or some other obtuse and tenuous subject. We had gone out back of the house before breakfast to pick blueberries. Right away I heard, "What's a 'Unitarian,' Daddy?" I ate two more berries, slowly. Then I drew Alice around the bush to a branch low on the opposite side. "Here's some nice ones, Honey. And don't eat them now, save 'em in the pan for breakfast." I was trying to put breakfast as well as the bush between Alice and the Unitarians.

"You eat them, Daddy. What's a Unitarian?"

"A Unitarian doesn't believe very much in God or Jesus, and thinks each person ought to figure his religion out for himself."

She stopped munching and opened her eyes wide, "What do they have a church for then?"

That was the kind of thing Alice said. I was glad no one was around, especially a Unitarian, to hear this latest. Already a cynic about grown-ups, treating them as though they were half child, the rest bluff, she did not wait for my answer. From down behind the bush she wanted to know, "Daddy, what's 'evolution'?"

At the moment I wasn't worried. Not since the monkey trial in Tennessee at least has evolution been controversial. Furthermore, my work is in biological science, testing baby food for palatability. And, in college I had a course in genetics. "Evolution is how plants and animals came to be," I began, reaching in my memory for the page in the genetics text. "Every living thing is different from every other living thing from accidental changes in their heredity, in their genes. These accidental changes enable animals and plants to adjust to their environments, their surroundings. Since only a few of the many creatures that are born can live, the environment decides which changes survive. Accidental changes in genes are 'mutations;' the environment's deciding which changes are passed on to future generations is 'natural selection'."

I expected Alice would, for once, breath an overwhelmed "Oh." Instead, she stood up munching, pushed the thick curtain of long brown hair back from her blue eyes, stared at me a moment, and protested, "What's 'environment'?"

She had not capitulated to my over-all explanation, which I had quoted almost to the word from the college text; but her second question opened the way for the coup-de-grace: "That's easy," I snickered, "'environment' is everything outside the organism." As I went toward the house with the berry pan the boomerang hit:

"Daddy, are you 'environment'?"

Stunned, it was a minute before I realized the couping weapon was still in my possession. If you don't understand a concept, make it include everything including the skeptic. That way the person will think you are infinitely smarter than he. Also, since 'everything' includes the person, and he doesn't understand it, it is too scary to think about. So towering over Alice I intoned, "I suppose we are all 'environment'."

"But, Daddy, if you're environment, what decides whether you'll survive? I'm too little to decide about you, and you'll be too busy to decide about me."

I guess I felt sorry for the kid. "Let's talk about something else," Igrovled, throwing a protective arm around her neck. I felt protective -- not for long. Alice said, "If living things are an environment to otherthings, and living things change accidentally, then that is how environments change; wouldn't it be an awful mess?"

"It is an awful mess!"

"What's evolution then?"

I looked around for the answer. A movement in the garden registered subliminally, triggering a shot from the depths: "Listen, if rabbits had small teeth for eating carrots, and a rabbit happened to be born with big teeth, wouldn't it have a better chance to eat the food rabbits eat, to eat the food its brothers and sisters with small teeth couldn't eat? Or suppose a child had more brains than its parents," (I wished I'd stopped with rabbits) "it would probably grow up to have smarter children, wouldn't it? Natural selection means that every kind of creature improves at whatever it does. Living things become more and more intelligent."

Alice thought a while, brightened: "Then it's God. Natural selection is God."

Fortunately, or unfortunately, our maudlin conversation by the bush was terminated at that point by an incredible happening. Down the path from the carrot row ran a rabbit, ran a white rabbit on its hind legs, a white rabbit with very large front teeth set intriguingly amidst very worried mouth and eyes. "Dear me," panted the rabbit, "far less than 1 in 100 ... rather a lot of ..." The bunny sped by Alice, who, seeming not the least surprised, sped after it, while the end of the sentence trailed away, " ... destruction."

The rabbit of "Alice in Wonderland," I mused. A world-worried rabbit deserves a following; I was destracted enough to follow anything.

Alice had run through the garden into the meadow beyond. I could see her, flying hair, brown arms earnestly pumping, skip over the creek that drains our farm, a lissome, free spirit. Suddenly I found what I had not found for many a year, that it was a beautiful morning. The bright morning sun shot over the pines at the edge of the meadow, its shimmering whiteness joining the mists rising over the green grass, sparkling with dew. Here and there a spider web, woven in the night to catch a single moth or beetle, a gossamer brocade float-strung with pearls, waited the breeze that would carry it away. I realized I had not run like a child for a long time, and jumping the creek thought of a favorite passage from Walden:

> I have penetrated to those meadows on the morning of many a first spring day, jumping from hummock to hummock, from willow root to willow root, when the wild river valley and the woods were bathed in so pure and bright a light as would have waked the dead, if they had been slumbering in their graves, as some suppose.

Alice was waiting beneath a giant oak; at her feet was a cave which sloped sharply down, embraced by the tree's arching roots. "It's certainly large for a rabbit hole," I puffed. "Aren't you surprised at all, Miss Alice?" Alice smiled sweetly, a littly pityingly, "It's not an ordinary rabbit hold," she said. Perhaps it was the morning, or the child, or the berries miracle-fresh from sky and earth. Whatever it was, I did not hesitate when Alice disappeared, but with a deep breath and a shout plunged into the hole.

Spinning, Sliding, Falling -- I don't know why we didn't constantly bump noses and shins, for the light from the entrance grew very dim before the light from the other end, at the bottom, gave any help. We landed after a long straight-down fall. For some minutes I sat letting the

Niagara of impressions and released chemicals subside into a new pool of consciousness. While I stood brushing off the dirt, Alice tied a shoe, than got up pushing back the eternal hair. "I guess you're wondering what we're doing here," she drawled. My consciousness seemed okay. Alice took my hand: "C'mon, let's find out."

At first our eyes were dazzled by the magnificence of the place. Rising before us were many great trees like tents in an amusement park, and buildings of every shape, size and color under and among the trees. Next, we saw the huge sign blazoned over the roadway leading into the park. I was attempting to make a "D" or "W" for "Disneyland" or "Wonderland" from the "E" when Alice put an end to this adult sophism by reading ecstatically, "EVOLUTIONALDN." Smaller signs like political campaign stickers were plastered all over the columns of the "arch of triumph" entrance. We were to read their legends everywhere we went in Evolution-land. One declared, "The TOE was here." The other, "The TOE is watching you." We walked through the arch, I holding Alice's hand as much as she holding mine.

****

The first attraction was a fifty-by-fifty foot, white frame building, a red-and-white-striped awning shading wide windows on either side of the door, the false front above the roof bearing across its full length the words, "AUGUST'S SAUSAGE SHOP." Entering, we saw row on row of glass showcases filled with luscious sausages, "in the dab" and "in the gut" as my gentle old mother used to say. No one was at the counter toward the rear of the building, but from that direction a remarkable series of sounds issued at half-minute-or-so intervals. It sounded like: "click, thump, squeel, ya, ya." Alice couldn't see over the counter, so at her frantic bidding but with terrific strain on my sacro-iliac I set her on the top. To say "an amazing sight greeted us" is to declare the complete inexpressi-bility of language.

This part of the building was obviously a laboratory, and the tiers of small cages covering side and back walls indicated a biological laboratory. On the floor in the center of the area was a heavy, black-topped table, sink at one end. I am almost sure a dog, a big doberman pinscher with red face and black whiskers, was sitting at the table. It was dressed in a white coat, wore a filter mask over its mouth, or muzzle, and was making intricate measurements. As we watched, the Doctor -- let us call it that since it was a scientist -- the Doctor took a mouse from a cage, "click", inserted the tail into a tiny guillotine: "Thump." "Squeel." The "ya, ya) came from the Doctor as he measured the severed tail. Into a notebook went the notation, the tailless mouse went back to its cage, the tailed joined a pile like a mountain of spagetti.

"Poor mouse," cried Alice glowering at the Doctor.

"Ah!" he barked, (or should I say, he "said"?) "a customer."

Curiosity reinforcing her sympathy, Alice spoke again though obviously not the "customer" the Doctor wanted to give audience to: "Would you please tell me why you are hurting mice?"

Pulling the mask down off his face, the Doctor blew himself up with air becoming very red indeed. His spring from behind the table and bark almost catapulted Alice off the counter: "Experimenting!"

Having subdued his female adversary and observing my interest, he went on, whiskers scintillating: "I have been cutting off tails of mice for three hundred and sixteen generations. You can rest assured there is absolutely no transmission from soma to germ. I'll demonstrate." Taking a tail from the heap and a small caliper scale from his coat pocket, he laid them side by side on the counter. "The scale was set almost 70 years ago. It shows the exact length of the first tails. As you see, this tail is no shorter after three hundred generations of tailless parents."

"Terrific!" I breathed.

Alice said: "This tail is <u>longer</u> than the first tail. I never saw such a long tail."

"Would you like some nice somatic sausages this morning? I can grind you some fresh."

"You're just like my Daddy; please tell me why the tails are longer."

The Doctor moved his arms sideways and up as though letting go a stack of plates. "Very well, tails have gotten longer, tail muscles stronger. Why?" His gaze lifted to the ceiling, eyes misting under furry brows. "Such are the mysteries of science. Science will always have these details--'de-tails'" (he winked at me) "--for young scientists, young men that is, to work out." (Authoritatively brusque again.) "The point is, the tails haven't gotten shorter. That is the point because the principle of 'the immaculate germplasm' tells us that is the point! We must always decide which evidence is <u>significant</u>; the evidence which is significant agrees with the particular milieu. Anyone who accepts all the evidence, <u>as it is</u>, we..." Tossing the mouse tail he had demonstrated with back on the pile, the Doctor looked meaningfully from pile to Alice to pile. Then he leaned toward me, tongue rougishly lolling between giant canines. "What kind of sausage would <u>you</u> like?"

I did not think I would like mouse tail of Alice sausages even if it was for science. "How are the 'germinal' sausages this morning?"

Making fun of the Doctor was the furthest thing from my mind, however--"Don't be a fool, "he snapped, "they are too small. Besides, you can't eat germinal sausages, " --he was scratching himself with a hind leg--"they are immortal." I started to explain that the germinal sausages were for souvenirs, but the Doctor, eyeing another pile of somethings, little flaps of skin they seemed to be, interruped, "A different kind of meat perhaps?"

I swung Alice down from the counter. "Is that what I think it is! I knew that was part of the original work, but..." My voice went to a whisper, "Doctor, why use mouse tails when you can use prepuces ?"

He was furious. I don't blame him, a great scientist having to listen to a question like that. He fairly snarled, "Of course prepuces have been amputated for hundreds if not thousands of generations without disappearing. But, it was not done in the laboratory! The world will believe anything if you do it in the laboratory."

Seeking a route of escape I realized Alice had gone. Calling "thanks for showing us the lab and these splendid results" I rushed for the door. A last glance revealed the doberman, teeth bared and growling, reaching for another mouse.

****

The little girl, blue pinafore dress, black shoes, white ankle socks, graced by and gracing the bright morning, was standing before what resembled a parrot cage, a cylinder of bars topped by a conical roof, only five times as tall and round as Alice. She was holding the bars in paroxysms of mirth or fear, I couldn't tell which. I hurried toward her.

I always look for signs first. That is bad because you should see what it is for yourself before taking someone else's word for it. I guess it is a feature of our literarally superstitious culture--we don't believe it rains unless it says so in the newspapers. This sign read: "The Missing Link." Sure enough, inside was an ape, and I began to realize how very special an ape he was when Alice declared, "I think he's 'telling evolution'."

He was a dignified old fellow, not examining his excrement either; any ape can look dignified examining his excrement. No, the Evolutionland Ape was squatting in the middle of the concrete floor with something on the floor between his legs that he was pounding. Alice pointed above him. A great cluster--there must have been hundreds--of large gold watches hung on gold

chains from the peak nearly to the floor. Other gold objects hung half way down the chains. After several minutes' decipherment I realized the Ape was solemnly smashing watches with Phi Beta Kappa keys.

From under the crude though efficient watchsmasher, springs sprung and wheels wheeled about the enclosure. Intently following one of these marvels, the Ape pulled down into place from above his beetling brows a one-eye jewler's loop. Over the floor he scuttled, his magnifier glued to the crazily-hopping spring, his other end straight up. Alice alternately hid and peeked around my arm in embarrassed delight. Considerably less amused, I demanded, "What did you say he was doing?"

She managed to surress her giggles: "You told me those 'minutions'--what-you-call-'em? those changes in heredity, 'mutilations,' that's it! --you said they were accidents. Well, those are 'evolution watches,' and that is how you 'tell evolution.'"

Damn it! this was science. Or was it entertainment? Pretty clearly it was science and entertainment. How to expose the imposter? Why not get him to talk! A Phi Beta Kappa Ape might not be smart enough to converse, but one that could use that magnifier could surely talk. "Pardon me, sir..." --and the Ape did a disconcerting thing. He disappeared. Unfortunately, or fortunately as the reader's preference may be, Alice knew what to do.

"Man!" she exulted, crouching on her haunches and peering among the watch wreckage on the floor, "that last spring was real cool."

"Cool?" inquired the Ape, materializing in the cage in front of her.

"Like crazy, Clyde, like real outside."

The Ape stuck his head between the bars, staring at Alice's nose through his monicle from a distance of half-an-inch. "Mmm, Darwin was right, 'endless slight peculiarities.' Are you from Chatham or Albemarle? And from this far away I can't quite tell--are you a land or a marine iguana?"

She was my child; it was my theory. Materialized or not, the Ape was going to hear from me. "She is not an iguana, she is my daughter. Furthermore..."

Pulling himself up suddenly, the Ape rammed his magnifier at me almost smashing my nose. "Ah, then you are the land iguana. The marine iguana descended from the land form. Also you lack valve nostrils."

I pressed on, nostrils valveless and battered. "Furthermore, Darwin made a slight mistake about his 'endless slight peculiarities.' We now know that chromosome assortment has nothing to do with gene mutation. Furthermore..."

The living fossil reached out hairy arms full length. "If you'll step closer I'll give you a mutation. Land iguanas are so disgustingly primitive."

"Moreover, did you know that De Vries made the same mistake? Discovering Mendel's Laws of chromosome assortment, he declared that a completely different kind of change was needed in evolution. Then he based the 'gene mutation' on the same chromosome assortment, in evening primrose, that he was dismissing as 'not original,' merely 'fluctuating.' Then it was that H. J. Muller..."

Unable to improve my genes, the Ape had returned to Alice. "Too bad you and your pop have so little in common. Land iguanas are awful bores."

"'Squares' you mean."

Over the conspiracy of Ape and marine iguana, I pounded on to my fourth point-"corner" according to Alice. "As for the smashed watches linking Darwin and modern genetics, Muller did nothing of the kind. X-ray is ... ah, therapeutic in fruit flies. And it kills cancer." (O.k., it also causes cancer, but who knows? evolution may be a kind of cancer!) "X-ray brings order out of the gene's molecular chaos. The man who gave the Nobel Peace Prizes invented dynamite, but Muller didn't get the Prize for blowing up genes. After all ..." but the Ape had disappeared. From the cage's ceiling one of those stickers stared down at me: "The TOE is watching you."

It was all nonesense I knew. Yet as I took Alice's hand and we turned our backs on the 'Evolution-Telling Ape,' I couldn't help but feel my own mainspring was as shattered as the watches'. With my beguiling, doubting little Eve, "hand in hand with wandering steps and slow," I was taking my way out of Christendom's very last and scientific Eden. Still the companionship of the child was an Eden too, and the guilt or fear vanished as her ringing "Look!" announced our third wonder in Evolutionland.

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We were traveling on the right of the road that looped through the park from entrance and back. In the widening central island grew many old, wide-branching oaks. Approaching through this woods, banging into trunks, falling down, coming on, rebounding, coming on again, was a shambles of every structure ever devised in evolution, and more. To give an idea: the middle of the back mounted a single powerful wing, which, frayed from tangling with trees, flopped as when a chicken has its head cut off. The legs it walked

on were three misplaced, different-length stems resembling an ostrich's. Sticking out from all sides, never finding the ground, were assorted hoofed, clawed, pincered, suckered, flippered and spaded legs. The body was covered with scales, spines, shell, feathers, hair, warts and lime, in gobs, patches and spangles. The head--the part we would soon be most interested in--extended from the left side like an elk's head askew on a wall; closer inspection showed more resemblance to the head of a praying mantis. Upon attaining the road behind us, the shambles looked toward the entrance, then turned, or rather "re-disintegrated," in our direction.

We had gotten off the road to let it by, since it took almost the entire space what with its junk-yard bulk and undertain gait. We being on the head side when it drew abreast, it saw us and stopped. Its immense wing continued to flap, stirring up dust like a helicopter, making nearly as much noise, while side feet pawed, raked and clawed the air with the futility but not the coordination of the tube feet of an upturned starfish. What happened next was almost to be expected: "How do you do" announced a faint treble. The greeting would have been inaudible were it not simultaneous with another remarkable event. Having rotated the head to fix an eye on the wing, the creature spoke as that errant dorsal member raised itself and quivered its own courtly respects. The succeeding remarks were almost lost in the seraphic blast, but we were listening now, just out of range of the extra legs.

"Have you seen--I'm looking for--I'd appreciate if you know of--an environment I might fit." A sanguine attention, a pure and deathless hope shown through the prismatic hardness of a pair of glasses inches thick. A straining hoof grazed my ear as I bent to listen. "There's really no such thing as a monster, you know. Or should I say, we are all monsters. We are only accidents surviving by fitting <u>anything</u> outside us. An environment is just where <u>any</u> accidental organism lives. It doesn't matter what you are, if you'll look long enough you'll find your environment."

Oh, the faith and fighting heart! Oh, the noble dream! As though divining my need, the creature shock all over causing its spectacles to fall on the ground. That is, one pair fell off; what I had taken for very thick lenses were actually pair on spectable pair worn one on top of the other. "'The Theory spectacles,' piped the mantis head, "Wear them when you need to see in the proper and hopeful light. Look for an environment for me; any world, any dimension. Adapt, survive, multiply--what fun! Must find a mate, mustn't I?--legs do get in the way, but--these difficulties can be overcome--mates are environment too. Keep surviling!"

The myriad aerial legs began furious uncoordinated gyrations, the single wing flopped in the last extremity of headless ensanguination, and after considerable scratching in diverse times and spaces, the three ostrick legs again launched "the monster in search of an environment." As it dispersed around the walled enclosure ahead, our next stop in Evolutionland, I noticed Alice making disrespectful faces.

"Great!" she fumed, "'We are all monsters.' If everything has an environment, 'environment' doesn't mean a thing. There can't be 'selection.'"

"Honey," I sighed rubbing away a tear, "there are things little girls just don't understand."

TITLE: An Attempted Replication of studies by Halas *et al.* and by Thompson and McConnell*

AUTHOR: Paul Cornwell**
Planaria Research Group
University of Michigan

The conclusion of Thompson and McConnell (1955) that true classical conditioning can be demonstrated in the planarian has recently been questioned by Halas, James & Stone (1961). The main point made by these authors is that, since light elicits a variety of responses in the naive planarian, the responsiveness to light by the experimental Ss in the Thompson and McConnell study may have been due to non-associative factors. However, control groups (particularly group LC) run by Thompson and McConnell seemed to show that this was not the case.

The apparent discrepancy between these two studies may very well be due to procedural differences. Thompson and McConnell trained all Ss individually and recorded responses made in each 3-sec. stimulation period during the acquisition session. Halas *et al.* however trained Ss *en masse* without recording responses. The dependent variable in the Halas *et al.* study was the number of responses made by each S during a 25-trial extinction series administered on the day following the acquisition session. It would seem very likely that the results of both studies are reproducable. While responses to light may habituate over a 150-trial session (Thompson and McConnell), one day may be a sufficient length of time for nearly complete dishabituation to take place (Halas *et al.*).

The present study was undertaken to determine if the above procedural differences could account for the apparent discrepancy between the results of Thompson and McConnell and those of Halas *et al.* In the present study Ss were studied individually, responses made by each S being observed during both acquisition and extinction sessions.

METHOD

Subjects:

Thirty-three planaria of the Tigrina stock B (University of Michigan) were used. All of these Ss were descendents of a single large Tigrina that had been cut into several sections about a year prior to the start of the present experiment. Originally it was planned that 30 to 40 Ss would be trained, and then the data would be analyzed in order to determine how many additional Ss should be run. Prior to training Ss were kept in the large stock room and were given food occasionally. Ss were usually given their final feeding three days prior to training. From one to four Ss were tested per week during each week of the spring (1961) semester.

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**This research was performed while the author was a USPHS fellow at the University of Michigan.

Apparatus:

The apparatus used was that described by McConnell, Cornwell and Clay (1960). This apparatus is quite similar to the one used by Halas et al. (1961) and the one used by Thompson and McConnell (1955). Only one of the two troughs described in the paper by McConnell et al. was used at a time to insure against any possible spread of light. The room illumination was provided by a single overhead 50-w. bulb. The light was directed at the opposite the experimental trough. Between the training and extinction sessions Ss were kept in a small room under conditions of constant dim illumination, provided by a shaded 50-w. bulb.

Procedure:

Throughout the experiment only one S was studied at a time. After being placed in the experimental trough by means of a small brush, each S was given a 5-min. acclimation period. The training period consisted of three 50-trial blocks, with a 5-min. rest period interposed between blocks. This is essentially the same procedure used by Halas et al. and by Thompson and McConnell. It should be noted again, however, that Halas et al. trained six Ss at a time, while in the present study and that of Thompson and McConnell all Ss were trained individually.

Upon completion of the training period, S was transferred to an individual aquarium. The same S was re-tested under extinction conditions on the following day, after a lapse of from 20 to 26 hours. The apparatus used during the extinction training was the same one as was used during acquisition. A total of 100 extinction trials was administered each animal in two 50-trial blocks separated by a 5-min. rest period.

Ss were assigned to one of six groups randomly, with the restriction that each replication of all six groups had to be completed before the next replication was begun. An attempt was made to keep the experimental conditions as close as possible to those employed by Halas et al. The duration of the CS (light) presentation was 3-sec., that of the UCS (shock) presentation was 1-sec., and the UCS was presented only during the final second of CS presentation.

The groups were treated as follows:

GROUP	EXPERIMENTAL CONDITIONS	
	Acquisition	Extinction
E	Paired light & shock	Light only
LC	Light only	Light only
SC_h	Shock only	Light only
NLNS	Neither light nor shock	Light only
NRC	Neither light nor shock	Neither light nor shock
SC_{tm}	180 trials given in 6-trial blocks (5 shock trials followed by 1 light-only (test) trial)	Light only

The experimental conditions for the first five of the above groups are the same as those employed by Halas et al. for their similarly labeled groups. The sixth group (SC_{tm}) was added in an attempt to replicate the conditions employed by Thompson and McConnell for their shock control group.

All groups except group NRC were divided in half so that the effect of CS intensity could be studied. For one-half of the Ss in each group a 200-w. CS was used (two 100-w. bulbs); a single 7.5-w. bulb was used for the CS for Ss in the other half of each group.

In summary, the main procedural differences between the present study and that of Halas et al. were as follows:

1. In the present study Ss were run in the same trough on two consecutive days while in the Halas et al. study two different troughs were used.

2. In the present study all Ss were trained individually, while in the Halas et al. study Ss were trained in groups of six.

3. The mean intertrial interval in the present study was 21.8 sec.; that of Halas et al. was over 30 sec.

4. Ss that failed to move during the experimental session were prompted to move by gentle stroking with a fine brush in the present study. Halas et al. prompted their sluggish animals to motion by use of water currents set up by a pipette.

On the first three of the above points the procedure employed in the present study was the same as that employed by Thompson and McConnell.

RESULTS

Figures 1 and 2 show the results obtained with groups E, LC, and NLNS for both acquisition and extinction sessions. The only procedural difference between the two figures is that of CS intensity. In Fig. 1 the CS intensity was 200-w.; in Fig. 2 it was 7.5-w. It should be noted that the results of the Thompson and McConnell study are also reproduced in Fig. 1.

In Table 1 the results from group SC_{tm} are compared with those obtained by Thompson and McConnell with their similarly treated shock control group. Also shown in Table 1 are the results of another independent replication of this shock control group run at the University of Michigan in the fall of 1960. In Fig. 3 the results obtained by Halas et al. are compared with those obtained in the present study.

Since the N in each group of the present study was very small (3) and the variances obtained were quite large, no statistical tests were run. Therefore all of the results presented should be considered as tentative only.

TABLE 1

Mean Responses on First 15 and Last 15 Test Trials for Group SC (CS Intensity = 200-w.)

Study	N	First 15 Test Trials	Last 15 Test Trials	Diff.
Thompson & McConnell	10	5.6	4.6	-1.0
Cornwell (Fall 1960)	6	4.2	1.9	-2.3
Present Study (Group SC_{tm})	3	3.0	5.3	2.3

TABLE 2

Mean Responses on First 15 and Last 15 Test Trials for Group SC (CS Intensity = 7.5-w.)

Study	N	First 15 Test Trials	Last 15 Test Trials	Diff.
Present Study (Group SC_{tm})	3	3.3	3.0	-0.3

DISCUSSION

Despite the small N used in the present study, there would seem to be definite indications that the data of Thompson and McConnell and those presented by Halas et al. are complementary. The present study would seem to show that both of the above studies are replicable, and replicable with the same animals. In Fig. 1 it is quite evident that the data obtained by Thompson and McConnell are very similar to those obtained in the present study with the 200-w. CS. In Fig. 3 it can be seen that there is, with one exception, quite good agreement between the results of the first 25 extinction trials run in the present study and the results obtained by Halas et al. under fairly similar conditions (200-w. CS). The one exception is in group LC. In the present study the responsiveness of this group was considerably below that shown by the Ss in the Halas et al. study. This apparent discrepancy may very well be due to sampling error in the present study, since the Ss run under 7.5-w. CS showed a response level more similar to that shown by the Ss in the study by Halas et al.

The main difference in results obtained under the two light intensities is in the acquisition curves of Group E. While at the 200-w. intensity this group shows a definite trend in performance which might be interpreted as learning, no such trend is to be found in the results obtained with 7.5-w. Perhaps this is not too surprising since Corning has shown that while planaria can acquire a classically conditioned response when the CS intensity is as low as 7.5-w., it usually takes 500 or more trials for the animals to reach criterion.

SUMMARY

Six groups of planaria were studied in an attempt to replicate the results obtained by Halas *et al.* and those obtained by Thompson and McConnell.

Although no definite conclusions can be drawn because of the large variances and small N in the present study, it would seem that the major results of both Thompson and McConnell and of Halas *et al.* have been replicated.

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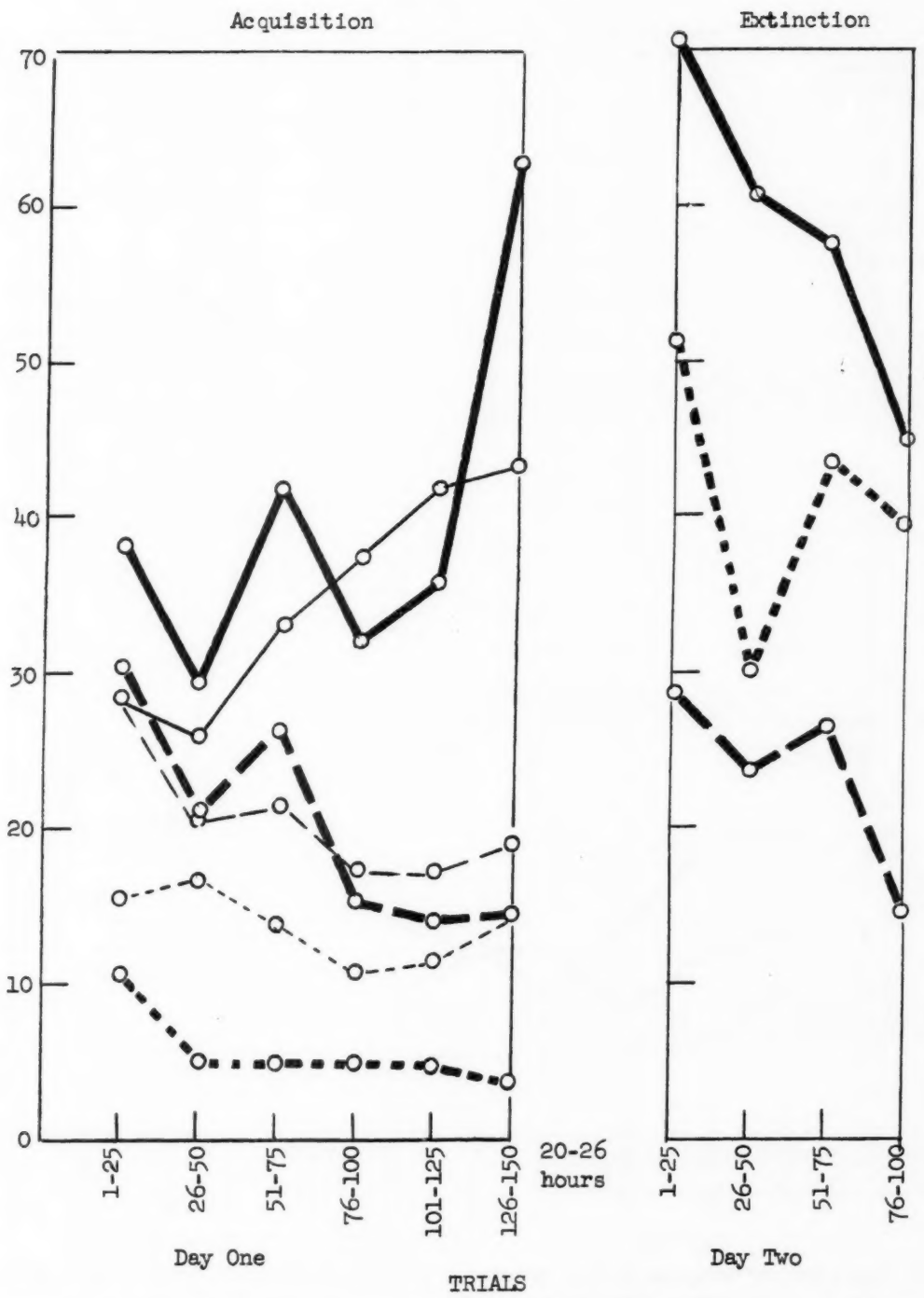
Whether you're upwardly mobile, social climbing, peregrinating, or just being restless, as you move about the world, please keep the DIGEST informed of your changes in address.

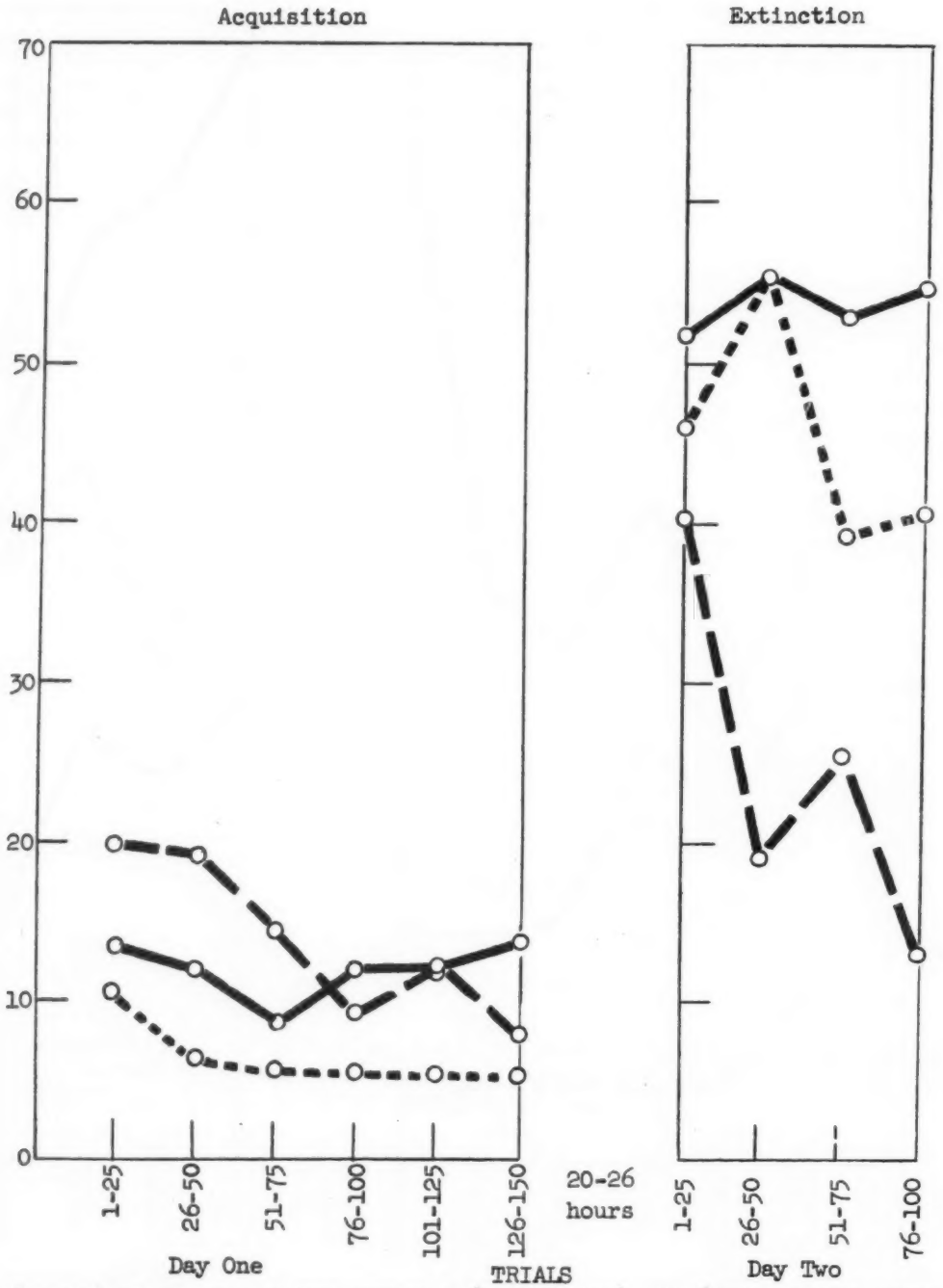
Figure 1



Percent of responses for groups E(solid line), LC(interrupted line), and NLNS (dotted line). The thick lines are those of the present study (N = 3 per group), the thin lines those of the Thompson and McConnell study (N = 10 per group).

Figure 2

CS INTENSITY = 7.5-w.

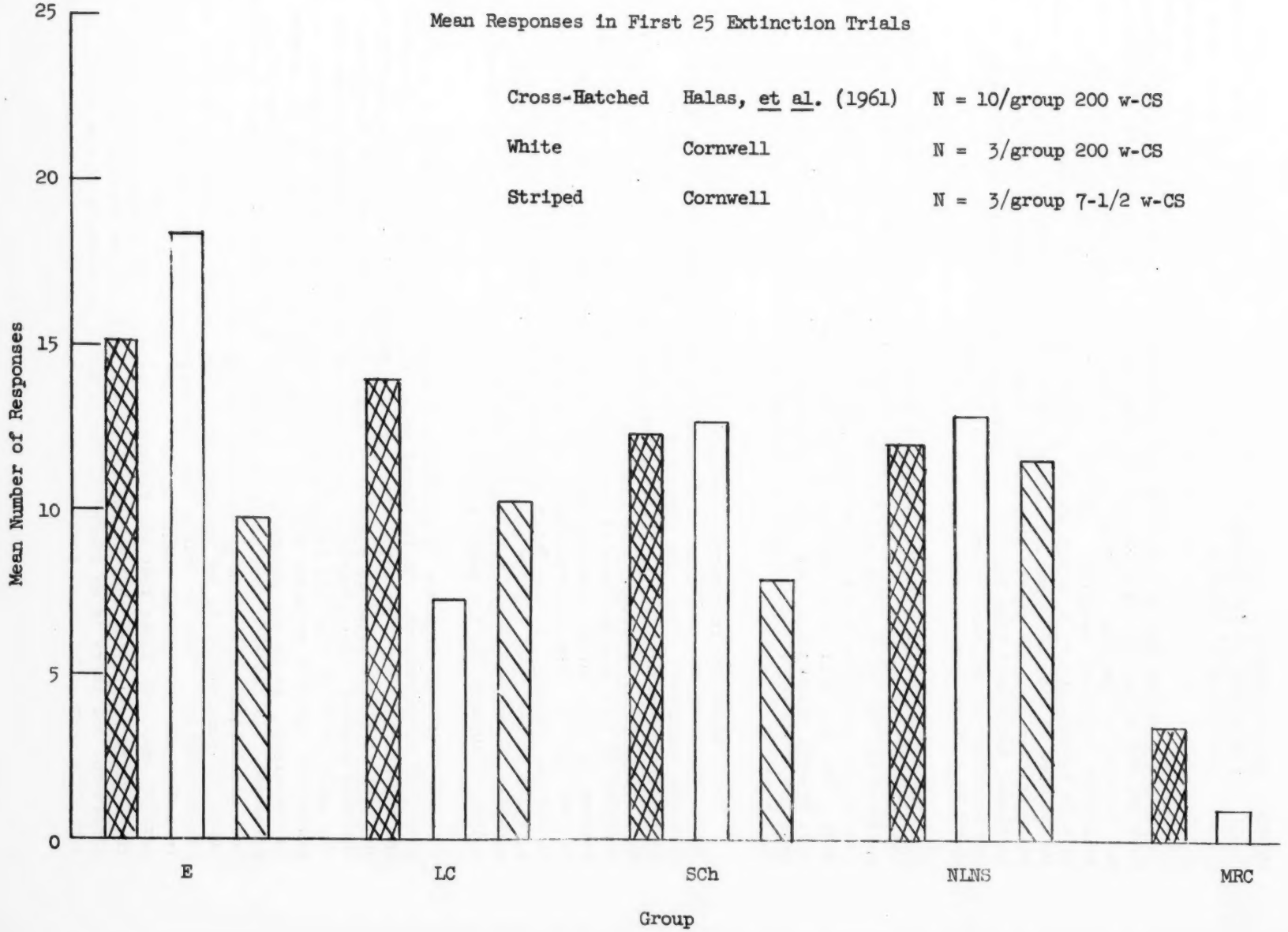


Percent of responses for groups E (solid line), LC (interrupted white line), and NLNS (dotted line).

Figure 3

Mean Responses in First 25 Extinction Trials

Cross-Hatched	Halas, et al. (1961)	N = 10/group 200 w-CS
White	Cornwell	N = 3/group 200 w-CS
Striped	Cornwell	N = 3/group 7-1/2 w-CS



TITLE: The Dear Departing

AUTHOR: Robert Sommer
Department of Psychology
University of Alberta

Since everyone has to leave his job sometime, why is leaving so difficult, both for the person who is going and for the people he leaves behind? The formal ceremony of departure, whether it is a dinner or a presentation, is relatively short and painless. Yet the last weeks drag by interminably, and this is accompanied by a change in the person's status. He becomes a ghost in the eyes of his colleagues, existing wraith-like in a twilight state, being neither here nor there, present nor absent. He gradually withdraws his emotional investment from his job and surroundings and everything about him now seems unreal and meaningless. His colleagues appear to him as marionettes moved by strings from which he has been cut loose.

The way a departing person becomes a ghost was made very clear to a friend who left her job as head of a small department. Several people applied for her position and one of them entered her office to inquire about the details. She completely ignored my friend's presence and went directly to the secretary's desk. She then started interrogating the secretary about the details of the job, salary, responsibilities, etc. In exasperation the secretary finally told her, "Why don't you ask Mrs. X; she certainly knows more about the job than I !"

Every departure represents a failure of the system since it means that either the wrong man was hired or the company was not good enough to hold a superior candidate. Thus, the employees who remain develop a series of rationalizations and myths to protect themselves against feelings of rejection. Some consider any departure a betrayal of the firm and its principles. Others desperately seek explanations of why the person is leaving, regardless of the reasons he lists on his resignation form. More often than not, these explanations pin the blame on some single policy that has been a source of irritation within the company. If Joe Smith has tried to get a particular program accepted by the firm, and for some reason Bill Brown in another department resigns, Joe Smith may proclaim loudly that Bill Brown is leaving because of a lack of support for Loe Smith's policies.

The continued presence in an office of someone who has resigned is a source of embarrassment. He is viewed as a corpse that has not yet been buried and may start to smell at any moment. People prefer that he keep out of sight and make as little noise as possible. He may find himself shunned by others who fear possible contamination. The moment of ultimate rejection occurs when the employee finds that his files have been lost or sent elsewhere. This erases all tangible evidence that the person ever existed as far as the company is concerned. It is not uncommon for a departing employee to find that his personnel file has been lost or is incomplete. This means he will have a difficult task establishing his claim to vacation time or pension rights. A man often takes his personnel file for granted until that fateful day when he hands in his resignation. Then he learns that the personnel officers have been busy recording all the trivial events in his career and overlooking all the important ones. He finds that the clerk has dutifully filed the memo he wrote three years ago saying that he was taking his child to the dentist for the afternoon, but there is no record of the payments he hasmmade to the superannuation plan. The final settlement always seems to favor the company - records

of money owed the employee tend to disappear while records of money owed the company remain intact and legible.

When a man announces he is leaving, this drastically reduces his political influence in the company. Whereas previously he was able to bargain from strength, his associates know that he is now unable to fulfill any type of contractual agreement. Even his ability to requisition things like typewriters, filing cabinets, ar wastebaskets, is diminished He begins to feel that he is surrounded by a flock of vultures just waiting for him to go so they can take his secretary, office and filing cabinet. He finds that his subordinates are looking around for other positions, especially if they have been loyal to him and feel that his successor may want to bring in his own people. Others begin forming new alliances simply to insure that the change will not affect their present status. A few subordinates may be intent on getting the incumbent's job, so for them the interval between the announcement of his resignation and his departure is one of feverish activiey.

Although a man's power to grant favors diminishes with his resignation, it should not be assumed that everyone is free to insult him. He must still be treated kindly and sometimes with more deference than before. Because he is leaving, he is no longer bound to uphold the public image of the company. He is free to speak his mind and, if he feels like it, make like a canary. In any organization there's a tremendous difference between the external picture of the way things are done and the way things really happen on the inside This is not to say that all organizations are corrupt, but only that the external image is rarely a true reflection of the inner workings. If an ex-employee decides to sing, he can do incalculable damage to the company. This is why some corporations give a bonus to a departing employee which he doesn't receive until several months afterwards, and this gift is contingent upon his doing nothing to injure the firm and its competitive position.

If it is an executive who is about to leave, everyone in his department, except those who want his job, begins to slack off. The whole tempo of the department slows down, and his assistants and even his stenographer become sloppy. They start to come late, leave early, forget to empty the wastebasket or water the office plant, neglect to pick up the mail or open letters. The most unfortunate aspect of this letharge is that it occurs at a time when the man himself is working feverishly to finish up all the programs he started. He knows that most of his undertakings will be abandoned once he leaves, so he strives heroically to put them in some order. While he is working nights and weekends, his assistants and secretary are cutting their identification with him and his programs. Essentially, it comes down to this - who wants to take orders or type letters for a ghost ? It is very difficult for a ghost to bestow rewards; the most he can do is promise that he won't haunt the premises. Because of a sentimental attachment and a vague desire to speed the ghost on his journey into the next world, his secretary and assistants do perform the necessary work of the office, albeit without vigor or zeal.

The influence of the departing person in picking his successor is almost nil. He is unable to bargiin with other executives since he has nothing to offer except his silence, which may be enough to get him a going-away present but not much more. In fact, nothing is more disheartening for a person than to meet his prospective successors. None smems worthy or even capable of wearing his mantly. Even if the applicant is well motivated and qualified, his interests will probably be very different from those of the incumbent. Furthermore, the applicants will try to avoid close ddentification with the incumbent

or his policies. They want to avoid committing themselves to policies prematurely, and secondly, to avoid inheriting any ill will that existed toward the incumbent. The new person wants to make his own alliances and too close an association with a predecessor can create a sterotype that will be hard to break. Thus while the person's colleagues treat thim as a ghost, the prospective applicants for his job treat him as a leper. He finds that he sits alone at coffee and is shunned at various public gatherings, except insofar as there are professional malcontents in the firm or other people who are leaving.

The departing employee is often obsessed with thoughts of his going-away present and farewell speech. He knows that he has been contributing his quarters and half-dollars for other people, and now finally his turn has come. What will he receive - a gold watch ? a suitcase? a silver dish? This will be the real measure of the esteem in whcih he is held by his colleagues If he fears that others will forget to aake up a collection for him, he begins to drop veiled hints during coffee about the kinds of things he needs. If this doesn't work, he starts making remarkd like "I wonder if they're going to give me gold cufflinks like they gave old Jason, ha ha." An equally important consideration is what he should say in his farewell speech. He is tempted to really blast the company and expose all the inefficiency and nonsence he has witnessed. This would be the ideal time to state his case since he will haee the floor to himself. Yet because he is leaving, the struggles of the past few years and the quarrels and rivalries of the firm mean very little to him. It hardly semms worthwhile to devote any effort to improving something that has become so unreal and distant. His farewell address will follow the familiar pattern - "Thank you all for the gold watch and the nice times we've had. I'll be thinking of you often and don't forget to visit us if you are ever in town." He will graciously accept his gift and officially join the ranks of the dear departed.

TITLE: Factors Retarding and Accelerating Regeneration in Planaria*

AUTHOR: Jack Denaro
281 Ribbon Street
Franklin Square, New York

Introduction

Having worked with the elementary regeneration of Planaria for a year, I wished to continue to experiment with them during my junior year. However, now I wished to do something original with them, that is, not to duplicate someone else's experiments. After searching through many references, I noted that none of them had dealt with the acceleration and retardation of the regeneration rate. Therefore, this was the topic I decided to work with.

Purpose

The purpose of the experimentation was to find out what effect, if any, the varying of the environment of the Planaria through the addition of chemicals and the varying of temperature of the water had upon the amount of time needed by a Planarian to regenerate a specific area of its body, in this case the area ranging from the uppermost part of the pharynx to the tip of the head.

Materials used

Many of the materials used are the same as one would use for the ordinary regeneration of Planaria. One would need Planaria, a dissection knife, a small sheet of glass, pond water, and a multitude of bottles. In addition to this, a fairly accurate scale, milliliter graduate, incubator, refrigerating unit, stainless steel picks, and a light producing apparatus are needed.

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*For further information on this study, see "Worms and Things" in this issue.

Procedure

An individual Planarian was taken from one of the jars and allowed to extend on the glass. Then a horizontal cut would be made just above the pharynx. Meanwhile, a solution of the chemical in filtered pond water would be prepared, or the temperature control chamber would be set to the desired temperature. Immediately after cutting, both halves of the Planarian would be immersed in the bottle containing the "new environment", and observations upon their growth would be made daily.

Pigmentation

In order to determine if the pigmentation was complete, the experimental Planarian was compared with a normal Planarian that had not regenerated over a period of two months. The subjects were compared side by side under a 25X lens. When both appeared equal in pigmentation, it was determined that the once translucent head had become completely pigmented.

Completion of eyes

The same method was used in this case as was used to determine the state of pigmentation. When the eyes were just beginning, a small response to light was noticed. Day by day, the eyespots increased in completeness until the black spots stood out clearly. When this occurred, it was determined that the development of the eyes was complete.

Naturally, if a greater degree of completeness was noted on the day after the original decision was made, the new figure would take prominence over the old.

Test for reaction to light

The Planarian was again allowed to extend on the sheet of glass, and when it had stopped movement, a bare flashlight bulb (powered by two 1-1/2 volt batteries) was suspended directly above the head area (5 mm) flashed on. If the eyes were not complete, the Planarian would remain dormant. However, when the eyespots began to appear, the Planarian showed a definite negative phototropism and quickly moved our of range of the light. This was tried three times consecutively to make sure that the effect was caused by the light and not something else.

Number of subjects in each experimental condition

In each experimental condition, two Planaria were present, in separate bottles of course. One (coded I) was considered the master Planarian and this was the one from which the official observations were made. Another Planarian (coded II) was a follow-up, cut on the same day, in order to check on the progress made by the master. If any wide discrepancy in number of days to reach the milestones (± 20% of I), the experiment was done over. This occurred only with the experiments concerning extreme heat and chloral hydrate.

Weights and proportions of substances used in experiments

Letter code

A.	Normal conditions...no additive
B.	Temperature change...no additive
C.	Temperature change...no additive
D.	2ml 200 proof grain alcohol to 150ml H_2O
E.	300mg thyroxin to 600 ml water
F.	30mg caffeine to 1 liter water
G.	2ml (one normal) paregoric to 700ml water
H.	100mg chloral hydrate to 500ml water
I.	Temperature change...no additive
J.	Temperature change...no additive
K.	Temperature change...no additive

Experiment Code

Letter	Planaria subjected to
A	Normal conditions
B	Moderate cold
C.	Extreme cold
D.	Grain alcohol
E.	Thyroxin
F.	Caffeine
G.	Paregoric
H.	Chloral hydrate (failure)
I, J, K.	Heat (failure)

Composite Chart for the Time of Regeneration of Planaria

Below may be found a composite chart of development of each subject, according to the time at which they reached specific objectives, or milestones. These objectives, mainly taken from the observations of section two, are the response to light, beginning of auricles, beginning of eyes, completion of eyes, first ingestion of food, first pigmentation, and day of completion.

Subject	Light	Beg. Aur.	Beg. Eyes	Finish Eyes	Ingestion	Pigmentation	Finish
A	5	8	9	11	13	22	32
B	9	14	15	17	19	23	38
C	..	..	..	..	..	..	..
D	6	6	10	12	13	17	22
E	4	5	7	9	12	12	16
F	7	6	7	9	12	10	17
G	4	4	6	8	11	10	16

Numbers above are concerned with how long a period of time, measured in days, the Planaria(n) needed to reach that point of development.

Discussion and Conclusions

The control Planarian, subject A, developed in the following order. First, the response to light was noticed on the fifth day. Then, on the eighth day, the auricles began to form. On the ninth, the eyes began, and were completed on the eleventh. Following this, on the thirteenth, the subject ingested food for the first time. The beginning of pigmentation for the otherwise completed head began to take place on the twenty-second day. Finally, on the thirty-second it was completed.

Subject B was subjected to a moderate decrease in temperature, to about 13°C, plus or minus a degree. This was found to lengthen the time for the regeneration for this subject to thirty-eight days. It can also be noted that the accomplishment of the "milestones" took place in the same order as those of the control. Also, each of the milestones took a considerably longer time to achieve. Considering the total time of regeneration of the control to be 100%, that of subject B is 117.8%. Thus, it can be said that a moderate decrease in temperature slows down the regeneration process.

Subject C was subjected to temperatures ranging about the 5°C mark. This was found to lengthen the time for the complete regeneration of this subject to an undetermined number of days. After the first day, absolutely no growth took place. As long as the temperature remained that low, the Planarian remained dormant. At no time would it ingest food or respond to light. Thus, after observing it for twenty-one days without significant result, the experiment was discontinued. Therefore, it may be concluded that extreme cold completely halts the regeneration process in Planaria.

Subject D was placed in an environment of one part two hundred proof grain alcohol to seventy-five parts of water. Used over a period of time, grain alcohol is at first a stimulant, then a depressant.

This subject closely followed the order of the milestones of the control, except for the response to light and the development of the eyes. It took one day longer for this subject to show a response to light, and one day longer to complete and begin the eyes. It can be noted at this point that alcohol can produce blindness in human beings if too much is consumed. A parallel may be found here. As two subjects were placed in the same environment, but different tubes, practically the same results were observed for its sister Planarian. It can also be noted that the most growth took place on the day after the alcohol content in the water was replenished.

This subject was completed in twenty-two days as compared to thirty-two for the control. Again, considering the regeneration time for the control to be 100%, the regeneration time for this subject would be 68.8%.

Subject E was placed in an environment of dessicated thyroxin and pond water.

Thyroxin, the hormone product of the thyroid gland, acts as a governor in the oxidation of body cells, the metabolism of the body increasing as the amount of thyroxin increases.

The milestones for this subject were reached in the same order as those of the control, but were reached in a much shorter period of time. The biggest surge of speed was noticed between the first ingestion of food and the completion. For the control, nine days were required between the first ingestion of food and the pigmentation. For subject E, pigmentation took place on the same day as the ingestion. Again, ten days were needed for the control to become complete from the first day of the initial darkening. For this subject, only four days were needed. Also, this subject needed only one-half the regeneration time of the control in order to complete.

The subject in experiment six was subjected to an environment of caffeine and water.

In moderate amounts, caffeine is non-toxic and stimulating.

This subject did not follow the milestones in the same order as the control. To begin with, instead of responding first to light and then beginning the growth of the auricles, the opposite took place here. Altogether, this subject took one day longer to respond to light, but began its auricle development one day sooner. It made the time from the auricle beginnings to the response to light in one day, while the control took three days. Another mixup in the order of milestones was that this subject began to darken two days before ingesting food. On the other hand, it took nine days for the control to go from its first ingestion to the initial pigmentation. This subject was completed in seventeen days as compared to the thirty-two days for the control. The regeneration time for subject F then would be 53.2%.

Subject G was placed in an environment of paregoric (one normal) and water.

A As long as this opiate is in continued use, it acts as a stimulant, but once its use is stopped, it takes the form of a depressant.

This subject followed the same order for most of the milestones as did the control. The only difference was that this section began to darken before it ingested food. In this way it can be likened to subject F. Also, the first response to light and the beginning of the growth of the auricles came on the same day. This was unusual as it has never happened before in the two years that I have been experimenting with these animals. This subject took sixteen days to regenerate, the same period of time as that of subject E, and one-half that of the control.

Experiments that repeatedly failed

One of the experiments that failed time after time was the attempt of keeping Planaria in an environment of increased heat. After many tries, it was noted that Planaria do not adapt to high temperatures. Considering 21°C to be normal, a prolonged rise of four to five degrees Centigrade is sufficient to kill the subject. At first, I tried to keep them in a Pyrex jar at 36°C in an incubator. Finding this to be unsuccessful, I gradually reduced the temperature to 32°C, 30°C and finally to 25°C, always with the same result. At 36°, the subject succumbed within four hours, at 32°, seven hours, at 30° for one day, and at 25° for three days. Thus, it can be concluded that Planaria cannot exist at moderately high temperatures.

Also, Planaria cannot exist in an environment of chloral hydrate and water. One hundred milligrams to one half liter of water was sufficient to kill the subject within six to nine days, providing that the solution was changed every day. This experiment was tried three times, with the subjects dying one the ninth, sixth, and seventh days, respectively. It was also noticed during the time that the subjects were alive that they fell far behind the control in development. For the first trial, after nine days, the subject was only equal to the fourth day of the control in development. It showed no response to light, did not form auricles, nor did it begin development of the eyes.

Conclusions

I. The regeneration of Planaria can be speeded up or slowed down through the use of chemicals or varying temperatures.

- A. A moderate decrease in temperature slows down the regeneration time to a small extent.
- B. A pronounced decrease in temperature slows down the regeneration process to the point at which the subject becomes dormant.
- C. The addition of two hundred proof grain alcohol to the water dramatically cuts the regeneration time.
- D. The addition of thyroxin to the water in which the Planarian is living apparently halves the time of regeneration.

E. The addition of caffeine to the water nearly halved the regeneration time.

F. The addition of paregoric to the water achieves the same result as the caffeine.

G. The heating of the water that the Planarian is living in to the extent of four to six degrees Centigrade over room temperature (22°C) is sufficient to kill them.

H. The addition of chloral hydrate to the water slows down the regeneration time a great deal, but also kills the subjects within six to nine days.

TITLE: Some Comments On An Addition to the Theory of Psychosexual Development'

AUTHOR: Sigmund Fraud *
Department of Physiochemical Sociology
University of Vienna
Vienna, Arkansas

* Real name of this noted psychologist NOT available on request

In previous writings concerning the stages of psychosexual development, there has been a serious error of ommission With more careful observation, an important stage can be discerned, occurring between the anal and phallic stages. This is the period when the libido is localized primarily in the mucous linings of the nose, and I shall therefore refer to it as the nasal stage

Altho in most cases it is not quite as drastic as toilet training, nasal training cannot be overlooked. At one extreme we have the mother who places great emphasis on the child's ability to clear his nose, thus creating a nasally active character. At the other extreme we have the mother who provides no nasal training, and even resorts to nose drops - the result being a nasally passive adult character.

The child who attempts auto-erotic nasal stimulation, by the insertion of the finger into one of the nostrils, may be met with a nose bleed This is a traumatic experience, similar in kind to castration anxiety, and may lead to repression of nasal impulses and fixation at the nasal stage of psychosexual development.

Let us condiser now the manifestation in the adult of the nasal character. The 1st sign of such a person is compulsive nose-picking. You will frequently find him with a finger in one of his nostrils. The compulsive nature of this act results from a repression of its erotic meaning, as a result of the nasal castration anxiety which develops from nosebleeds in early childhood The nasal character is also the type of person who is constantly interfering in the affairs of others, hence the expression "sticking his nose into other people's business" and ther term "nosey". He considers himself superior, and is characterized as "turning up his nose" at various things

Altho to a lesser extent than in persons fixated at the nasal stage, we find residues of nasal eroticism in all adults. One example of this is the ceremony with which a handkerchief (frequently decorated) is carried in the pocket of the male, in the purse of the female. Further evidence of the sexual significance of this may be seen in the sharp distinction commonly made between the "man's" handkerchief and the "lady's" handkerchief .

Another illustration of a manifestation of residues of nasal erotic impulses in the adult is seen in the expression concerning the cutting off of one's nose to spite one's face - a clear manifestation of nasal castration anxiety

Nasal eroticism is not characteristic of a single culture, but may be found universally. Note, for example, the custome of rubbing noses in love-making among the Eximos.

Finally, in children's stories, we find a confusion between the nose and the phallis. This is particularly evident in Pinocchio.

A CASE: Hans was a German pilot in World War II During a routine bombing mission, his plane was intercepted by an enemy fighter and hit several times. He began to go into a nose dive All sorts of anxiety manifestations followed. He broke into a cold sweat and began to tremble Fortunately, his co-pilot kept notes on these reactions, which were recovered from the wreckage. Now why should Hans have shown these reactions during the nose dive: Can it be that the word "nose" had some special meaning tor him? When he was four he was caught by his father in the act of kissing his mother His father was jealous and punched him in the nose, which bled profusely Although this incident was repressed at the time, in this stressful situation his latent anxiety about his nose became manifest.

TITLE: Maze Learning in Planaria*

AUTHOR: Barb Humphries
Planaria Research Group
Department of Psychology
University of Michigan

A study designed to test for maze-learning in planaria is now in progress, and the initial results indicate clearly that the worms can successfully learn this sort of task.

The mazes used were simple plastic T-mazes set on a base so that the end of each arm just reached the water level of the bowl in which the worm was kept. Four mazes, each having one white arm and one black arm, one of the arms rough-surfaced, the other smooth surfaced, were employed. Each maze had a clear-plastic approach strip 1 3/8" long and 3/16" wide; the arms were each 1 3/8" long and 5/16" wide. The approach strip and the arms were grooved to a depth of 1/16". Both arms sloped slightly downward from the choice point.

In running a worm, uniform trails of water were laid on the maze with a brush, and the worm was prevented from reaching the water in the bowl when he chose the incorrect arm. This was done either by prodding the worm with a brush or by having the trail of water too short to reach the water level (systematic testing showed that the worms' choices were not influenced by the length of the water trail). After each trial the maze was wiped dry; occasionally the mazes were rinsed in dilute HC1.

Typically a worm preferred one arm with a frequency of 8 out of 10 trials; this preference was tested for and initial training was always to the opposite arm. A perfect trial was one in which the worm immediately chose the correct arm and reached the bowl; the criterion for having said that a worm learned a task was the completion of 9 perfect trials in 10, or better. The worms were ordinarily given 6-8 trials daily; thus in some cases the criterion was reached over several days. Inter-trial interval was 3-4 minutes.

The study was conducted in a room with just one light source which was centered with respect to the maze.

Ten Dugesia dorotocephala from Turtox Co. were run; of these, six have successfully learned at least one task and the remainder all show improvements. The results are summarized below.

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*This research was supported by grants to J. V. McConnell by the AEC and by NIMH.

WORM 1:

1. Learns at 45 trials.
2. Reverses at 155 trials.
3. Learns same color, opposite side (i.e., different maze) immediately - 10 trials.
4. Learns one color on two mazes presented randomly at 25 trials.
5. Learns one color on four mazes presented randomly at 17 trials.

WORM 2:

1. A tail dropped by #1 at trial 34 in step (2) of #1. Run three weeks later; prefers arm to which #1 was being trained.
2. Learns in 20 trials.
3. Reverses at 78 trials.
4. Cut immediately.
5. Regenerated tail run ten days later -- scores 9/9 immediately on task learned in step (3).
6. Regenerated head run eleven days later -- scores 9/10 immediately on task learned in step (3).
7. Regenerated head run sixteen days after cutting on different maze scores 9/9 immediately on same color opposite side.

WORM 3:

1. A tail dropped by regenerating tail of #2 two days after cutting.
2. Choosing "learned" arm 2/10 trials ten days later, i.e., twelve days after original cutting.

WORM 4:

1. Learns at 57 trials.
2. Learns same color, opposite side at 13 trials.

WORM 5:

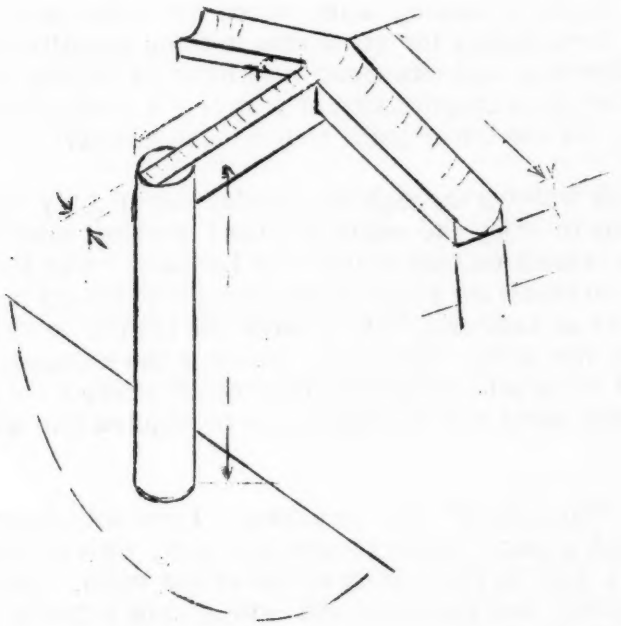
1. Learns at 79 trials.
2. Learns same color, opposite side at 13 trials.

WORM 6:

1. Learns at 55 trials.

WORM NUMBER	TRIALS SO FAR	CORRECT RESPONSES IN FIRST 50 TRIALS	CORRECT RESPONSES IN SECOND 50 TRIALS
7	130	6	14
8	130	4	11
9	95	3	11
10	80	7/40	16/40

Thus all ten worms show signs of learning, in varying degrees. Evidently the use of these and similar maze problems provides a potentially useful technique for increasing both the range and the rigor of studies of learning in planaria.



TITLE: Learning Theory*

AUTHOR: James V. McConnell**
Planaria Research Group
Department of Psychology
The University of Michigan

I am writing this because I presume He wants me to. Otherwise He would not have left paper and pencil handy for me to use. And I put the word "He" in capitals because it seems the only thing to do. If I am dead and in hell, then this is only proper. However, if I am merely a captive somewhere, then surely a little flattery won't hurt matters.

As I sit here in this small room and think about it, I am impressed most of all by the suddenness of the whole thing. At one moment I was out walking in the woods near my surburban home. The next thing I knew, here I was in a small, featureless room, naked as a jaybird, with only my powers of rationalization to stand between me and insanity. When the "change" was made (whatever the change was), I was not conscious of so much as a momentary flicker between walking in the woods and being here in this room. Whoever is responsible for all of this is to be complimented -- either He has developed an instantaneous anesthetic or He has solved the problem of instantaneous transportation of matter. I would prefer to think it the former, for the latter leads to too much anxiety.

Yes, there I was walking through the woods, minding my own business, studiously pretending to enjoy the outing so that I wouldn't mind the exercise too much, when the transition took place. As I recall, I was immersed in the problem of how to teach my class in beginning psychology some of the more abstruse points of Learning Theory when the transition came. How far away and distant life at the University seems at the moment! I must be forgiven if now I am much more concerned about where I am and how to get out of here than about how freshmen can be cajoled into understanding Hull or Tolman.

Problem #1. Where am I? For an answer, I can only describe this room. It is about twenty feet square, some twelve feet high, with no windows, but with what might be a door in the middle of one of the walls. Everything is of a uniform gray color, and the walls and ceiling emit a fairly pleasant achromatic light. The walls themselves are of some hard material which might be metal since it feels slightly cool to the touch. The floor is of a softer, rubbery material that yields a little when I walk on it. Also, it has

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** We wish to thank Harry Altshuler for many favors connected with this story and with all the others.

a rather "tingly" feel to it, suggesting that it may be in constant vibration. It is somewhat warmer than the walls, which is all to the good since it appears I must sleep on the floor.

The only furniture in the room consists of what might be a table and what passes for a chair. They are not quite that, but they can be made to serve this purpose. On the table I found the paper and the pencil. No, let me correct myself. What I call paper is a good deal rougher and thicker than I am used to, and what I call a pencil is nothing more than a thin round stick of graphite which I have sharpened by rubbing one end of it on the table.

And that is the sum extent of my surroundings. I wish I knew what He has done with my clothes. The suit was an old one, but I am worried about the walking boots. I was very fond of those boots -- not because of any sentimental attachment nor because they had done me much good service, but rather because they were quite expensive and I would hate to lose them.

The problem still remains to be answered, however, as to just where in the hell I am -- if not in hell itself?

Problem #2 is a knottier one -- why am I here? Were I subject to paranoid tendencies, I would doubtless come to the conclusion that my enemies had kidnapped me. Or perhaps that the Russians had taken such an interest in my research that they had spirited me away to some Siberian hideout and would soon appear to demand either cooperation or death. Sadly enough, I am too reality oriented. My research was highly interesting to me, and perhaps to a few other psychologists who like to dabble in esoteric problems of animal learning, but it was scarcely startling enough to warrant such attention as kidnapping.

So I am left as baffled as before. Where am I, and why?

And who is He?

2

I have decided to forego all attempts at keeping this diary according to "days" or "hours." Such units of time have no meaning in my present circumstances, for the light remains constant all the time I am awake. The human organism is not possessed of as neat an internal clock as some of the lower species. Far too many studies have shown that a human being who is isolated from all external stimulation soon loses his sense of time. So I will merely indicate breaks in the narrative and hope that He will understand that if He wasn't bright enough to leave me with my wristwatch, He couldn't expect me to keep an accurate record.

Nothing much has happened since I began this narrative, except that I have slept, been fed and watered, and have emptied my bladder and bowels. The food was waiting on the table when I awoke last time. I must say that He has little of the gourmet in Him. Protein balls are not my idea of a feast royal. However, they will serve to keep body and soul together (presuming, of course, that they are together at the moment). But I must object to my source of liquid refreshment. The meal made me very thirsty, and I was in the process of cursing Him and everybody else when I noticed a small nipple which had appeared in the wall while I was asleep. At first I though that perhaps Freud was right after all, and that my libido had taken over control of my imagery. Experimentation convinced me, however, that the thing was real, and that it is my present source of water. If one sucks on the thing, it delivers a slightly cool and somewhat sweetish flow of liquid. But really, it's a most undignified procedure. It's bad enough to have to sit around all day in my birthday suit. But for a full professor to have to stand on his tip-toes and suck on an artificial nipple in order to obtain water is asking a little too much. I'd complain to the Management if I only knew to whom to complain!

Following eating and drinking, the call to nature became a little too strong to ignore. Now, I was adequately toilet-trained with indoors plumbing, and the absence of same is most annoying. However, there was nothing much to do but choose a corner of the room and make the best of a none too pleasant situation. (As a side-thought, I wonder if the choosing of a corner was in any way instinctive?). However, the upshot of the whole thing was my learning what is probably the purpose of the vibration of the floor. For the excreted material disappeared through the floor not too many minutes later. The process was a gradual one. Now I will be faced with all kinds of uncomfortable thoughts concerning what might possibly happen to me if I slept too long!

Perhaps this is to be expected, but I find myself becoming a little paranoid after all. In attempting to solve my Problem #2, why I am here, I have begun to wonder if perhaps some of my colleagues at the University are not using me as a subject in some kind of experiment. It would be just like McCleary to dream up some fantastic kind of "human-in-isolation" experiment and use me as a pilot observer. You would think that he'd have asked my permission first. However, perhaps it's important that the subject not know what's happening to him. If so, I have one happy thought to console me. If McCleary is responsible for this, he'll have to take over the teaching of my classes for the time being. And how he hates teaching Learning Theory to freshmen!

You know, this place seems dreadfully quiet to me.

3

Suddenly I have solved two of my problems. I know both where I am and who He is. And I bless the day that I got interested in the perception of motion.

I should say to begin with that the air in this room seems to have more than the usual concentration of dust particles. This didn't seem particularly noteworthy until I noticed that most of them seemed to pile up along the floor against one wall in particular. For a while I was sure that this was due to the ventilation system -- perhaps there was an out-going airduct there where this particular wall was joined to the floor. However, when I went over and put my hand to the floor there, I could feel no breeze whatsoever. Yet even as I held my hand along the dividing line between the wall and the floor, dust motes covered my hand with a thin coating. I tried this same experiment everywhere else in the room to no avail. This was the only spot where the phenomenon occurred, and it occurred along the entire length of this one wall.

But if ventilation was not responsible for the phenomenon, what was? All at once there popped into my mind some calculations I had made back when the rocket boys had first proposed a manned satellite station. Engineers are notoriously naive when it comes to the performance of a human being in most situations, and I remembered that the problem of the perception of the satellite's rotation seemingly had been ignored by the slip-stick crowd. They had planned to rotate the doughnut-shaped satellite in order to substitute centrifugal force for the force of gravity. Thus the outer shell of the doughnut would appear to be "down" to anyone inside the thing. Apparently they had not realized that man is at least as sensitive to angular rotation as he is to variations in the pull of gravity. As I figured the problem then, if a man aboard the doughnut moved his head as much as three or four feet outwards from the center of the doughnut, he would have become fairly dizzy! Rather annoying it would have been, too, to have been hit by a wave of nausea every time one sat down in a chair. Also, as I pondered the problem, it became apparent that dust particles and the like would probably show a tendency to move in a direction opposite to the direction of the rotation, and hence pile up against any wall or such that impeded their flight.

Using the behavior of the dust particles as a clue, I then climbed atop the table and leapt off. Sure enough, my head felt like a mule had kicked it by the time I landed on the floor. My hypothesis was confirmed.

So I am aboard a spaceship!

The thought is incredible, but in a strange way comforting. At least now I can postpone worrying about heaven and hell -- and somehow I find the idea of being in a spaceship much more to the liking of a confirmed agnostic. I suppose I owe McCleary an apology -- I should have known he would never have put himself in a position where he would have to teach freshmen all about learning!

And, of course, I now know who "He" is. Or rather, I know who He isn't, which is something else again. Surely, though, I can no longer think of Him as being human. Whether I should be consoled at this or not, I have no way of telling.

I still have no notion of why I am here, however, nor why this alien chose to pick me of all people to pay a visit to His spaceship. What possible use could I be? Surely if He were interested in making contact with the human race, He would have spirited away a politician. After all, that's what politicians are for! Since there has been no effort made to communicate with me, however, I must reluctantly give up any cherished hopes that His purpose is that of making contact with genus homo.

Or perhaps He's a galactic scientist of some kind, a biologist of sorts, out gathering specimens. Now, that's a particularly nasty thought. What if He turned out to be a physiologist, interested in cutting me open eventually to see what makes me tick? Will my innards be smeared over a glass slide for scores of youthful Hims to peer at under a microscope? Brrrr! I don't mind giving my life to Science, but I'd rather do it a little at a time.

If you don't mind, I think I'll go do a little repressing for a while.

4

Good God! I should have known it! Destiny will play her little tricks, and all jokes have their cosmic angles. He is a psychologist! Had I given it due consideration, I would have realized that whenever you come across a new species, you worry about behavior first, physiology second. So I have received the ultimate insult -- or the ultimate compliment. I don't know which. I have become a specimen for an alien psychologist!

This thought first occurred to me when I awoke after my latest sleep (which was filled, I must admit, with most frightening dreams). It was immediately obvious that something about the room had changed. Almost at once I noticed that one of the walls now had a lever of some kind protruding from it, and to one side of the lever, a small hole in the wall with a container beneath hole. I wandered over to the lever, inspected it a few moments, then accidentally depressed the thing. At once there came a loud clicking noise, and a protein ball popped out of the hole and fell into the container.

For just a moment a frown crossed my brow. This seemed somehow so strangely familiar. Then, all at once, I burst into wild laughter. The room had been changed into a gigantic Skinner Box! For years I had been studying animal learning by putting white rats in a Skinner Box and following the changes in the rats' behavior. The rats had to learn to press the lever in order to get a pellet of food, which was delivered to them through just such an apparatus as is now affixed to the wall of my cell. And now, after all of these years, and after all of the learning studies I had done, to find myself trapped like a rat in a Skinner Box! Perhaps this was hell after all, I told myself, and the Lord High Executioner's admonition to "let the punishment fit the crime" was being followed.

Frankly, this sudden turn of events has left me more than a little shaken.

5

I seem to be performing according to theory. It didn't take me long to discover that pressing the lever would give me food some of the time, while at other times all I got was the click and no protein ball. It appears that approximately every twelve hours the thing delivers me a random number of protein balls -- the number has varied from five to fifteen so far. I never know ahead of time how many pellets -- I mean protein balls -- the apparatus will deliver, and it spews them out intermittently. Sometimes I have to press the lever a dozen times or so before it will give me anything, while at other times it gives me one ball for each press. Since I don't have a watch on me, I am never quite sure when the twelve hours have passed, so I stomp over to the lever and press it every few minutes when I think it's getting close to time to be fed. Just like my rats always did. And since the pellets are small and I never get enough of them, occasionally I find myself banging away on the lever with all the compulsion of a stupid animal. But I missed the feeding time once and almost starved to death (so it seemed) before the lever delivered food the next time. About the only consolation to my wounded pride is that at this rate of starvation, I'll lost my bay window in short order.

At least he doesn't seem to be fattening me up for the kill. Or maybe he just likes lean meat.

6

I have been promoted. Apparently He in His infinite alien wisdom has decided that I'm intelligent enough to handle the Skinner-type apparatus, so I've been promoted to solving a maze. Can you picture the irony of the situation? All of the classic Learning Theory methodology is practically

being thrown in my face in mockery. If only I could communicate with Him! I don't mind being subjected to tests nearly as much as I mind being underestimated. Why, I can solve puzzles hundreds of times more complex than what He's throwing at me. But how can I tell Him?

7

As it turns out, the maze is much like our standard T-mazes, and is not too difficult to learn. It's a rather long one, true, with some 23 choice points along the way. I spent the better part of half an hour wandering through the thing the first time I found myself in it. Surprisingly enough, I didn't realize the first time out what I was in, so I made no conscious attempt to memorize the correct turns. It wasn't until I reached the final turn and found food waiting for me that I recognized what I was expected to do. The next time through the maze my performance was a good deal better, and I was able to turn in a perfect performance in not too long a time. However, it does not do my ego any good to realize that my own white rats could have learned the maze a little sooner than I did.

My "home cage," so to speak, still has the Skinner apparatus in it, but the lever delivers food only occasionally now. I still give it a whirl now and again, but since I'm getting a fairly good supply of food at the end of the maze each time, I don't pay the lever much attention.

Now that I am very sure of what is happening to me, quite naturally my thoughts have turned to how I can get out of this situation. Mazes I can solve without too much difficulty, but how to escape apparently is beyond my intellectual capacity. But then, come to think of it, there was precious little chance for my own experimental animals to get out of my clutches. And assuming that I am unable to escape, what then? After He has finished putting me through as many paces as He wishes, where do we go from there? Will He treat me as I treated most of my non-human subjects -- that is, will I get tossed into a jar containing chloroform? "Following the experiment, the animals were sacrificed," as we so euphemistically report in the scientific literature. This doesn't appeal to me much, as you can imagine. Or maybe if I seem particularly bright to Him, He may use me for breeding purposes, to establish a colony of His own. Now, that might have possibilities...

Oh, damn Freud anyhow!

8

And damn Him too! I had just gotten the maze well learned when He upped and changed things on me. I stumbled about like a bat in the sunlight for quite some time before I finally got to the goal box. I'm afraid my performance was pretty poor.

9

Well, it wasn't so bad after all. What He did was just to reverse the whole maze so that it was a mirror image of what it used to be. Took me only two trials to discover the solution. Let Him figure that one out if He's so smart!

10

My performance on the maze reversal must have pleased Him, because now He's added a new complication. And again I suppose I could have predicted the next step if I had been thinking along the right direction. I woke up a few hours ago to find myself in a totally different room. There was nothing whatsoever in the room, but opposite me were two doors in the wall -- one door a pure white, the other jet black. Between me and the doors was a deep pit, filled with water. I didn't like the looks of the situation, for it occurred to me right away that He had devised a kind of jumping-stand for me. I had to choose which of the doors was open and led to food. The other door would be locked. If I jumped at the wrong door, and found it locked, I'd fall in the water. I needed a bath, that was for sure, but I didn't relish getting it in this fashion.

While I stood there watching, I got the shock of my life. I mean it quite literally. The bastard had thought of everything. When I used to run rats on jumping stands, to overcome their reluctance to jump, I used to shock them. He's following exactly the same pattern. The floor in this room is wired but good. I howled and jumped about and showed all the usual anxiety behavior. It took me less than two seconds to come to my senses and make a flying leap at the white door, however.

You know something? That water is ice-cold!

11

I have not, by my own calculations, solved no fewer than 87 different problems on the jumping stand, and I'm getting sick and tired of it. One time I got angry and just pointed at the correct door -- and got shocked for not going ahead and jumping. I shouted bloody murder, cursing Him at the top of my voice, telling Him if He didn't like my performance, He could damn well lump it. All he did, of course, was to increase the shock.

Frankly, I don't know how much longer I can put up with this. It's not that the work is difficult. But rather that it seems so senseless, so useless. If He were giving me half a chance to show my capabilities, I wouldn't mind it. I suppose I've contemplated a thousand different means of escaping, but none of them is worth mentioning. But if I don't get out of here soon, I shall go stark raving mad!

12

For almost an hour after it happened, I sat in this room and just wept. I realize that it is not the style in our culture for a grown man to weep, but there are times when cultural taboos must be forgotten. Again, had I thought much about the sort of experiments He must have had in mind, I most probably could have predicted the next step. Even so, I most likely would have repressed the knowledge.

One of the standard problems which any learning psychologist is interested in is this one -- will an animal learn something if you fail to reward him for his performance? There are many theorists, such as Hull and Spence, who believe that reward (or "reinforcement," as they call it) is absolutely necessary for learning to occur. This is mere stuff and nonsense, as anyone with a grain of sense knows, but nonetheless the "reinforcement" theory has been dominant in the field for years now. We fought a hard battle with Spence and Hull, and actually had them with their backs to the wall at one point, when suddenly they came up with the concept of "secondary reinforcement." That is, anything associated with a reward takes on the ability to act as a reward itself. For example, the mere sight of food would become a reward in and of itself -- almost as much a reward, in fact, as is the eating of the food. The sight of food, indeed! But nonetheless, it saved their theories for the moment.

For the past five years now, I have been trying to design an experiment that would show beyond a shadow of a doubt that the sight of a reward was not sufficient for learning to take place. And now look at what has happened to me!

I'm sure that He must lean towards Hull and Spence in His theorizing, for earlier today, when I found myself in the jumping stand room, instead of being rewarded with my usual protein balls when I made the correct jump, I discovered ...

I'm sorry, but it is difficult to write about even now. For when I made the correct jump and the door opened and I started towards the food trough, I found it had been replaced with a photograph. A calendar photograph. You know the one. Her name, I think, is Monroe.

I sat on the floor for almost an hour weeping afterwards. For five whole years I have been attacking the validity of the secondary reinforcement theory, and now I find myself giving Him evidence that the theory is correct! For I cannot help "learning" which of the doors is the correct one to jump through. I refuse to stand on the apparatus and have the life shocked out of me, and I refuse to pick the wrong door all the time and get an icy bath time after time.

It just isn't fair! For He will doubtless put it all down to the fact that the mere sight of the photograph is functioning as a reward, and that I am learning the problems merely to be able to see Miss What's-her-name in her bare skin!

Oh, I can just see Him now, sitting somewhere else in this spaceship, gathering in all the data I am giving Him, plotting all kinds of learning curves, chortling to Himself because I am confirming all of His pet theories. I just wish...

Almost an hour has gone by since I wrote the above section. It seems longer than that, but surely it's been only an hour. And I have spent the time deep in thought. For I have discovered a way out of this place, I think. The question is, dare I do it?

I was in the midst of writing that paragraph about His sitting and chortling and confirming His theories, when it suddenly struck me that theories are born of the equipment that one uses. This has probably been true throughout the history of all science, but perhaps most true of all in psychology. If Skinner had never invented his blasted box, if the maze and the jumping stand had not been developed, we probably would have entirely different theories of learning today than we now have. For if nothing else, the type of equipment that one uses drastically reduces the type of behavior that one's subjects can show, and one's theories have to account only for the type of behavior that appears in the laboratories.

It follows from this also that any two cultures that devise the same sort of experimental procedures will come up with almost identical theories.

Keeping all of this in mind, it's not hard for me to believe that He is an iron-clad reinforcement theorist, for He uses all of the various paraphernalia that they use, and uses it in exactly the same way.

My means of escape is therefore obvious. He expects from me confirmation of all His pet theories. Well, He won't get it any more! I know all of His theories backwards and forwards, and this means I know how to give Him results that will tear His theories right smack in half!

I can almost predict the results. What does any learning theorist do with an animal that won't behave properly, that refuses to give the results that are predicted? One gets rid of the beast, quite naturally. For one wishes to use only healthy, normal animals in one's work, and any animal that gives "unusual" results is removed from the study but quickly. After all, if it doesn't perform as expected, it must be sick, abnormal, or aberrant in one way or another...

There is no guarantee, of course, what method He will employ to dispose of my now annoying presence. Will He "sacrifice" me? Or will He just return me to the "permanent colony"? I cannot say. I know only that I will be free from what is now an intolerable situation. The chance must be taken.

Just wait until He looks at His results from now on!

II

FROM: Experimenter-in-Chief, Interstellar Labship PSYCH-145

TO: Director, Bureau of Science

Thlan, my friend, this will be an informal missive. I will send the official report along later, but I wanted to give you my subjective impressions first.

The work with the newly discovered species is, for the moment, at a standstill. Things went exceedingly well at first. We picked what seemed to be a normal, healthy animal and smattered it into our standard test apparatus. I may have told you that this new species seemed quite identical to our usual laboratory animals, so we included a couple of the "toys" that our home animals seem to be fond of -- thin pieces of material made from wood-pulp and a tiny stick of graphite. Imagine our surprise, and our pleasure, when this new specimen made exactly the same use of the materials as have all of our home colony specimens. Could it be that there are certain innate behavior patterns to be found throughout the universe in the lower species?

Well, I merely pose the question. The answer is of little importance to a Learning Theorist. Your friend Verpk keeps insisting that the use of these "toys" may have some deeper meaning to it, and that perhaps we should investigate further. At his insistence, then, I include with this informal missive the materials used by our first subject. In my opinion, Verpk is guilty of gross anthropomorphism, and I wish to have nothing further to do with the question. However, this behavior did give us hope that our newly discovered colony would yield subjects whose performances would be exactly in accordance with standard theory.

And, in truth, this is exactly what seemed to be the case. The animal solved the Bfian Box problem in short order, yielding as beautiful data as I have ever seen. We then shifted it to maze, maze-reversal and jumping stand problems, and the results could not have confirmed our theories better had we

rigged the data. However, when we switched the animal to secondary reinforcement problems, it seemed to undergo a strange sort of change. No longer was its performance up to par. In fact, at times it seemed to go quite berserk. For part of the experiment, it would perform superbly. But then, just as it seemed to be solving whatever problem we set it to, its behavior would subtly change into patterns that obviously could not come from a normal specimen. It got worse and worse, until its behavior departed radically from that which our theories predicted. Naturally, we knew then that something had happened to the animal, for our theories are based upon thousands of experiments with similar subjects, and hence our theories must be right. But our theories hold only for normal subjects, and for normal species, so it soon became apparent to us that we had stumbled upon some abnormal type of animal.

Upon due consideration, we returned the subject to its home colony. However, we also voted almost unanimously to request from you permission to take steps to destroy the complete colony. It is obviously of little scientific use to us, and stands as a potential danger that we must take adequate steps against. Since all colonies are under your protection, we therefore request permission to destroy it in tote.

I must report, by the way, that Verpk's vote was the only one which was cast against this procedure. He has some silly notion that one should study behavior as one finds it. Frankly, I cannot understand why you have seen fit to saddle me with him on this expedition, but perhaps you have your reasons.

Verpk's vote notwithstanding, however, the rest of us are of the considered opinion that this whole new colony must be destroyed, and quickly. For it is obviously diseased or some such -- as reference to our theories has proven. And should it by some chance come in contact with our other colonies, and infect our other animals with whatever disease or aberration it has, we would never be able to predict their behavior again. I need not carry the argument further, I think.

May we have your permission to destroy the colony as soon as possible, then, so that we may search out yet other colonies and test our theories against other healthy animals? For it is only in this fashion that science progresses.

Respectfully yours,

Iowyy

TITLE: A Failure to Confirm "Latent Learning in Earthworms"

AUTHOR: Jack Arbit[1]
Department of Neurology and Psychiatry[2]
Northwestern University Medical School
Chicago, Illinois

Recent theoretical (6, 7, 9) and experimental (2, 4, 8) reports evidence the continued interest and importance of latent learning in behavior theory. A study by Bharucha-Reid (1) reporting latent learning in earthworms is of significance because of its indications of the phylogenetic generality of the latent learning concept as well as the possible minimum neurological structures necessary for this complex type of learning.

The study by Bharucha-Reid, because of an inadequate control group, does not allow for a clear-cut interpretation of latent learning. In this study the control group ("reinforcement group") received five trials per day in a T-maze with negative and positive reinforcement: the experimental, latent learning, group was given twenty hours in a T-maze without reinforcement before starting to learn with the positive and negative goals. The latent learning group learned to choose the positive goal in significantly fewer trials than did the control group.

Haney (3), whose study represents a reference design for Thistlethwaite's Type II latent learning experiment (9), utilized a control group which was allowed to explore a maze differing in design from the test maze for the same number of hours as the experimental group explored the test maze. Without this type of control the importance of such factors as amount of handling, exposure to brush and flashlight (used to keep the worms moving during pre-training trials), as well as other variables cannot be equated in the groups. Karn and Porter (5) have previously noted the importance of pre-training procedures upon the performance of rats in latent learning experiments.

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[1]The author would like to express his appreciation to Mr. James P. McLean for assisting in the execution of this research.

[2]This research was performed while the author was on active duty at the U. S. Army Leadership Human Research Unit, P. O. Box 446, Ft. Ord, California.

Method

Subjects and Apparatus: The Ss were fourteen earthworms, Lumbricus terrestria, approximately six to seven inches long. The test apparatus was a T-maze constructed by fastening pieces of Lucite to a 2-ft. square piece of 1/2-in. plywood. The stem and crosspiece were each 25.4 cm. long, 2 cm. wide and 2 cm. high and were covered by a clear plastic hinged in such a way that any one of three equal-length segments could be uncovered independently. The stem of the maze was inserted into the crosspiece so that the left arm was 10.1 cm. and the right arm 15.3 cm. in length. The short, left, arm contained a one cm. piece of 2/0 sandpaper eight cm. from the choice-point followed immediately by two copper bell wires 1/4 in. apart through which a shock of one volt could be administered. This was the negative goal. The Lucite pieces which formed the maze were attached to the plywood base in such a manner that at the end of the long arm (positive goal) a beaker, containing moist earth and moss and wrapped in paper so as to keep the interior dark, could be attached. The floor of the maze was lined with paper toweling moistened in normal saline solution: a fresh lining was inserted before each animal was run as well as after the second and fourth trials on each day. The Ss were kept in a refrigerator between trials on successive days. A camel's-hair paint brush and flashlight were utilized to start and keep the Ss moving in the maze. Illumination was provided by a 15-watt bulb wrapped in cheese-cloth and centered three feet over the choice-point in the maze: during pre-training trials with the control group this light was centered over the runway.

Procedure: Seven animals were randomly assigned to the control group and seven to the experimental, latent learning, group. Two hours per day for ten days preceding learning in the test maze each S in the control group was allowed to remain in a closed runway 25.4 cm. long, 2 cm. wide and 2 cm. high. Each S in the latent learning group was allowed to remain in a T-maze similar to the test apparatus for the same period of time. During this time the T-maze had blocks placed at the openings so as to make it a closed maze. For both groups a stop-watch recorded the amount of time the subject was moving, and if at the end of each fifteen minute interval the time-in-motion was under five minutes the paint brush and flashlight were employed to insure five

minutes of motion in the subsequent fifteen minute interval. At the end of twenty hours of pre-training all Ss began learning trials with the negative and positive goal conditions in the test maze. Because of the influence of the diurnal cycle upon the activity and learning of the earthworm[3] all Ss were pre-trained and then run with the reinforcing conditions between 4 P. M. and 8 P. M. Ss were given five trials per day in the test maze. A correct trial was recorded when the worm reached the beaker without having received a shock.

Results and Discussion

The mean number of trials necessary for the experimental group to reach the criterion of seven consecutive correct trials was 27 (S. D. 5.74): the control group reached the criterion in 32 trials (S. D. 4.23). With a t of 1.9 this difference in means is not statistically significant (df 12; p .09).[4]

Thorpe (10) has indicated that latent learning, since it involves delayed response and transfer of training, is akin to what has been called exploratory learning and implies that animals which show latent learning are on a relatively high behavioral level. The findings of the present study confirm Thorpe's position in that earthworms, when the proper controls are employed, do not manifest latent learning.

The experimental group in the present study does not differ significantly (p .06) from the experimental group in the study by Bharucha-Reid (Mean 22; S. D. 1.00). The control group in this study, however, takes significantly (p .02) less trials to criterion than the control group in the earlier report (Mean 43; S. D. 5.29). Whether this is due to handling, runway practice or other variables involved in the pre-training experience of the control group in the present study has not been determined.

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[4] Additional data obtained after this paper was written raises the p value to .15.

Summary

A study reporting latent learning in earthworms was replicated employing a more adequate control group. The difference between the latent learning group and this control group in the number of trials necessary to reach the learning criterion did not reach statistical significance ($p < .09$).

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TITLE: A Cerebral Physiological Study on Conditioned Reflexes

AUTHOR: Hiroshi Akiyama
Osaka Technical College
Oseke, Japan

1. Positive Conditioned Refles.

In order to explain cerebrally and physiologically the phenomenon in Pavlov's experiments pertaining to the positive conditioned reflex, namely that if a dog is habitually given meat powder almost simultaneously with the sound of a flute, he secretes saliva and sucks his chops at the flute sound only, (even if given no meat,) we assume that, as shown in Fig. 1. only when auditory sense cells for a flute and gustatory sense cells for meat are excited simultaneously, not separately, "a new conditioned reflecting passage is formed in the sense area of cortex cerebri from the sense cells for hearin hearing a flute to these for tasting meat." To speak more minutely, we assume that, as shown in Fig. 2., only when the substances secreted by excitation from both ends of promections a and B whech compose the synapses in the conditioned reflecting passage from A to B, are mixed together, both, or either of, these ends make growth and the impulse conduction from A to B which was impossible till then becomes possible. However it is assumed that after having been secreted, either of these substances is easily and quickly dissolved or excreted and lose their functions. Moreover, though A, B and so on are illustrated as single cells in Fig. 2, they are in reality a multitude of cells and so must be illustrated as an area.

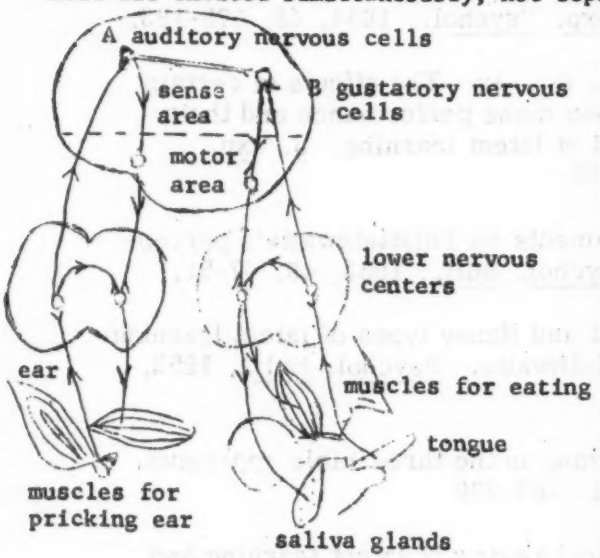


Fig. 1.

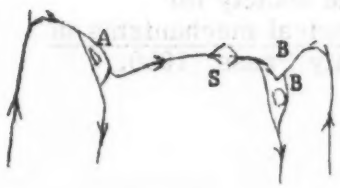


Fig. 2.

By the way, the hypothesis or law assumed as such is right so far as the various phenomenon jPavlov and his school have found can be explained by this law without self-contradiction and so far as conclusions deduced from this law can be proved with experiments. And when a new fact denying explanation by this law is found, this law becomes erroneous. However, the law must not be though radically and thoroughly erroneous, even if there were one phenomeonn which cannot be explained by it. In such a case what we, students, must do next with this law is not to abandon it entirely as erroneous, but to change it in part; namely we should not change in in it's general form or character but in its special one, so that we may explain this newly found phenomenon too and so as th expand the domain to which this law is applicable, and then elevate the degree of truth of this law. It is not to negate this law but to "aufheben"-----

If we observe this dog's phenomenon psychologically, we can see that on hearing the flute he "recollected" the last experience in which he was given meat soon after hearing the sound of a flute and ate it. That is, inferring from the last experience, he anticipated that as the flute was blown, meat would soon be given successively and he could eat it.---- Or it may be said that he was reminded of the taste of meat by hearing a flute. Namely pertaining to external world phenomena, he has known the fact that if a flute is blown, then meat will be given successively and with respect to himself the fact that, his digestive organs and skeltal muscles for eating will be excited. Namely, no sooner had he heard th sound of a flute that he "wanted" to eat meat. However, in such a psychological process, the dog neither discriminated "thinking" or "preliminary practice" from "practice", as human being does, nor "recollecting" from "presuming", nor "knowing" from "wanting" nor vice versa. Needless to say, he didn't think such a matter in language. The dog's ability to discriminate between such recollection, expectation, and others is not yet developed and, so to speak, in the state of germination. Thus we can call such a conditioned reflecting passage as recollecting, presuming, associating, wanting, etc, passage. Or when a dog acquires such a conditioned reflecting passage, we can say that the dog has memorized his past experience.

Moreover, what is worthy of our attention relative to this law is the following point. Namely we can infer that, thinking embryologically, as cerebrum and other various inferior nervous centers have originated from nervous tubes, such inferior animals as do not yet have their cerebra evolved may acquire a conditioned reflex, or such higher animals as have cerebra may form some exceptional conditioned reflecting passages in other inferior nervous centers. Next we can infer the following matters from this law.

Namely, the stronger the stimulus applied is, and the more frequently are the experiments repeated, the stronger becomes the conditioned reflex up to a certain limit. And even though the objectively measured intensity of an external stimulus, for instance the soud of a flute, remained constant, if the corporal condition (namely centripetal nervous excitements coming from sensory receptors or nervous ends distributed in the whole body, which are omitted in Fib. 2, and corporal liquid acting on nervous centers) changes, then, corresponding to them closely, the degree of excitement of sense cells in the cortex cerebri with respect to this external stimulus, namely the intensity of the sound of a flute felt subjectively, accordingly the unconditioned reflex too, for instance ear pricking, will be changed. For instance, the meat which is sweet enough to stir up the dog's appetite and incite his eating motion in his starving condition, will become an unpleasant object to be thrown up and the rejecting motion against it will be excited fittingly. Therefore even though the same conditioned and un-conditioned stimuli were used, the conditioned reflex that will be acquired will, if this corporal condition is changed, be changed accordingly. Or, even though the outer-action is determined, the conditioned reflex acquired will be undetermined so far as his corporal condition remains undetermined. Now, next, the corporal condition in which conditioned reflexes can be acquired most strongly by the same outer-stimuli is, it may be said, that in which susceptibility to outer-stimuli is highest, accordingly, that in which unconditioned reflexes stimulated by them are strongest, namely, the degree of awakening is highest. Namely it is a condition in which the sympathetic nervous center, suprarenal body, heart, lungs, liver and so on are excited, and the power to push blood into vessels, to breathe, to produce blood-glucose etc., are increased and moreover peristaltic movement and outer-secretion of digestive organs are decreased reversely. And it is a condition in which the blood-vesels in the motor-muscles, brain, the nervous centers and their end-organs are expanded, and the shrinking power of the blood-storage organs, liver, spleen, etc., and the flowing blood quantity in the former are increased; accordingly their renewal becomes better, their excitability conspicuous and the

dog's posture strained. Namely it is a condition, like a starving one in which the equilibrium of life renewal is threatened by, in this case, the progress of dissimilation. Accordingly it is a condition in which the stimulating threshold of accommodating motion reflexes (searching) and eating motions, to "renegate" or "restore" such a "negative" state according to the essential characteristic of life is remarkably lowered. Psychologically speaking, it is a negative or abnormal condition in which displeasure, pain or dissatisfaction are felt much. Therefore it is a condition accompanied with strong instinctive impulse or a desire to annihilate such displeasure and pain or to seek for pleasure and satisfaction. Accordingly conditioned reflexes which are liable to be formed most strongest and most variously have as their contents the accommodating special unconditioned reflexes corresponding to the speciality of awakening, for instance, either the hungry, sexual dissatisfaction or injury, namely appetitive, sexual or defensive and offensive motion. Desiring (or knowing) reflexes which can be easiest formed make it their objects to annihilate dissatisfaction. Reversely speaking, what can have him attain such objects as annihilate those dissatisfactions, displeasures and sufferings and to have him get satisfaction or pleasure, namely what can be the means pertaining to such objects, are to be made conditioned stimuli. And what has been mentioned above with respect to acquiring the conditioned reflex can also be said about its regeneration (or actualization). The conditioned appetitive reflex by the sound of a flute, even though the connecting degree of synapse S in Fig. 2 were constant, would hardly be regenerated in either the full stomach, injured or over-fatigued state, but in the starving condition. Under certain circumstances, he would feel like vomiting, getting angry or fleeing away (by a flute stimulus). Pertaining to this point sufficient attention must be paid of Pavlov's method in which the intensity of the conditioned reflex acquired is measured by the quantity of secreted saliva.

Moreover if conditioned reflecting passages are made from auditory sense cells corresponding to the conditioned stimulus, the sound of a flute, to gustatory sense cells corresponding to the unconditioned stimulus, taste of meat, in cortex cerebri on the occasion of their concurrent excitement, then similar conditioned passages must also be made vice versa. That is, the conditioned reflecting passages must form a circuit as shown in Fig. 3. Because as in the case of the meat stimulus, the sound of a flute is also an unconditioned stimulus, causing an ear-

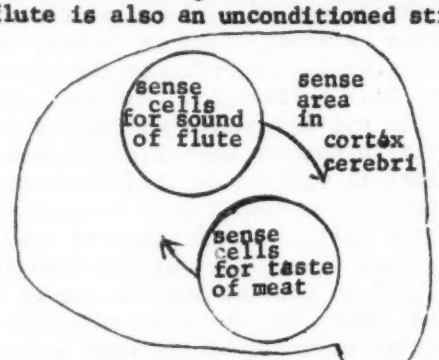


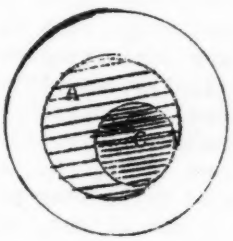
Figure 3

pricking motion unconditionally. Namely, if given meat, the dog will prick his ears, associating it with the sound of a flute (2, p. 111). However, the pricking-ear motion to search for something, by the sound of a flute, is rather suppressed unconditionally due to the eating motion stimulated by meat given simultaneously, and therefore, being acquired, it is also suppressed conditionally (compared with the occasion of being given no meat). Accordingly if observed externally, as Pavlov thought, the sound of the flute which can reflect a weaker "negative" displeasure and dissatisfactory condition and so a weaker "re-negative," restoring motion or impulse unconditionally becomes the conditioned stimulus and, on the

contrary, the stronger stimulus, meat, becomes the unconditioned stimulus, and the conditioned reflecting passage from the former to the latter will be formed. If we use an electric current on his skin instead of the sound of a flute and make its intensity stronger step by step, then it will be easily proved that the above-mentioned inference is right. Then, by inferring further, it will be concluded that "conditioned reflecting or associating passages are made among cells, in the sense area of the cortex cerebri, which are excited simultaneously." Not only among sense cells corresponding to abstracted stimuli, such as the sound of a flute and meat, but also among those corresponding to stimuli of the corporal condition and outer-circumstances must also be formed into conditioned reflection passages. Thus, for example, sense cells for tasting meat must be associated conditionally with those for the eating motion state. Therefore, it must be admitted that the unconditioned reflex is generally inseparable from the conditioned one.

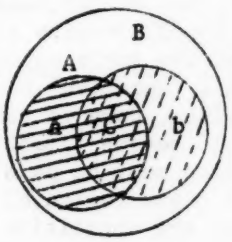
Thus, the cerebral law pertaining to the positive conditioned reflex is as follows:

"Special distribution of the excited area in the cortex cerebri which occurred in the past experiment (Or experience) will be regenerated if a part of the past excited area is stimulated later" (2, p. 111). In Fig. 4 after A was once excited, if part c is re-stimulated, then A will be excited again. But on the general occasion when the characteristics and the intensity of the conditioned reflecting passages in Area A before its first excitement are various and the distribution of the first excitement in area A is not uniform, regeneration of A by stimulus c will be relative. Namely the whole of A will not necessarily be excited.



sense area in cortex cerebri

Fig. 4



sense area in cortex cerebri

Fig. 5

Next, in Fig. 5, if the past excited sense area A (a + c) can be excited by re-stimulation of part c, than A must also be excited by new stimulus B (b + c) in which c is included. For instance, if appetite can be reflected by the conditioned stimulus, the sound of a flute, then it must also be reflected conditionally, even though the intensity may be different, by a new conditioned stimulus, the sound of a bell, which has a common excited sense area with the sound of a flute. The dog might mishear the sound of a bell for that of a flute or infer, in the form of germination or thinking, that it would also be the sign of a meal because of their resemblance. This is the phenomenon of generalization named by Pavlov (1, Lecture 7).

For example, suppose a man grasped a burning coal, burned himself, cried "alas!", cast is away and his face became tearful. In this case conditioned circuits will be formed in the cortex cerebri among the sense cells of seeing a burning coal, of hearing "alas," of the aching part burnt and, under certain circumstances, of seeing a tearful face or of seeing a casting motion. Afterwards, if he looks at a burning coal, hears a stranger shriek "alas!", or takes a look at his casting motion or his tearful face, he will, by associating them with the pain caused by the past burn, cry out "alas!" under some circumstances, and move or desire to flee. This is "imitation," "same-sentiment" or "sympathy."

2. Negative and Neutral Conditioned Feflexes

Next, we do not give the dog meat, but only blow a flute (the conditioned stimulus), which had fully been made positive by repetition of conditioning experiments in which the flute is accompanied by meat. In this case the appetite excited with expectation of meat, in which para-sympathetic nerves associated with the digestive organs, as well as sympathetic nerves, are excited, accompanied by the feelings ff pleasure and satisfaction, lose speedily the excitement of para-sympathetic nervous centers with a feeling of pleasure and changes into an abnormal exciting condition of the sympathetic nervous center only with a feeling of displeasure, the quantity of flowing blood, oxygen, blood-glucose, etc. and so the faculty of renewal, in the brain, nervous centers, sense receptors, skeltal muscles, etc. was lowered, seccessively, accompanied by a lowering of excitement of the heart, lungs, liver, etc. namely the degree ff awakening is lowered, briskness lost and fatigue appears, especially in the briin. Consequently, according to the characteristic of life, the unconditioned sleep reflex to "restore" this "negative" state is excited as accommodation. (1-Lecture 15) On this occasion, if the conditioned stimulation, the sound of a flute, is still condinued, the taste ff meat is continued, the appetitive reflex will, instead of becoming the sleep reflex, be changed reversely and become a sympathetic displeased motion to eat which was accommodative in such a case when his sleep is disturbed by meat. But the fatigue will be accumulated after all and the excieement of the sleeping nervous center will become superior. As a matter of course, conditioned reflecting passages are formed among the sense cells which are excited simultaneously corresponding to those processes. Namely, if we repeat such "nonstrengthening" (or "eliminating") experiemnts, the conditioned stimulus, the sound of a flute, will firstly form conditioned circuits aith the fatigued state resultigg from disappointment of the dog's expectation, and, acquiring the inhibiting reflex to eat, become a "negative" conditioned stimulus; and finally, acquiring the sleeping reflex, come to reflect nothing namely become a "Neutral" stimulus. (The dog will become almost indifferent to this stimulus.) (2,P.115-119) But, attention must be paod to the fact that, as shown in Fig. 6., even though the natural degeneration of synapse in the conditioned reflecting passage is premised, the appetitive and inhibitory reflex, though slight, will remain to be attached to the conditioned stimulus, the sound of a flute, because regeneration of a conditioned reflex has evvects to strengthen itself. Moreover it must also be noted that, on the occasion of the positive conditioned stimulus being condinued still more in a fatigued state of the brain, under certain aircumsannces, the "re-negative" (accommodative) reflex will take a form of anger or fear.

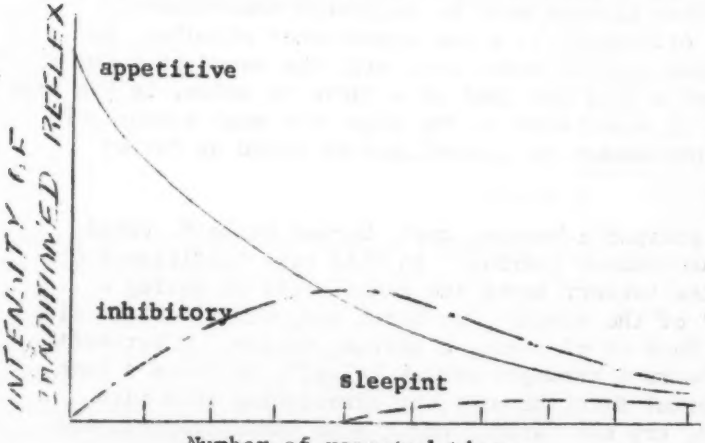
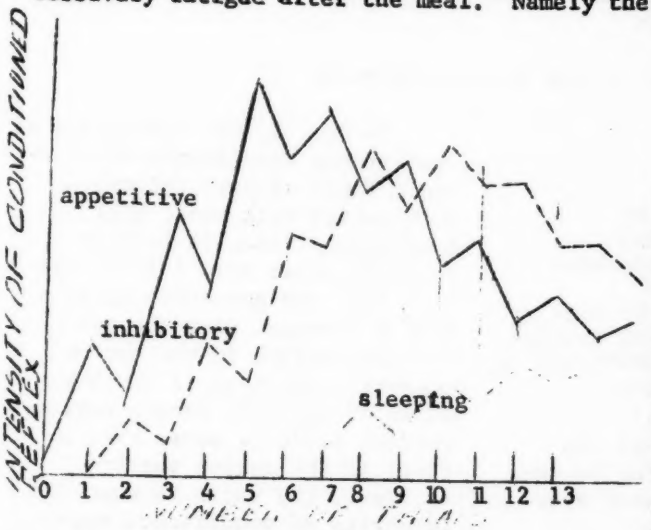


Fig. 6.

Namely, so far as the apecial "negative", abnormal state (for instance a starbing one) reflected by the positive conditioned stimulus is sucsessively annihilated by the movement reflected conditionally and through external materials closely related to the "re-negation" of thés "negative" state, namely as far as this positive conditioned reflex is accommodative, this will be strengthened to a certain and afterwards be maintained against its natural degeneration; but if the movements reflected by this come ..

to lead the animal to antother "negative" state (fatigue), namely if this posibly conditioned reflex does not becomes accommodative, it will, be acquiring a recovering reflex from fatigue, be made negative and successi ely neutral or indifferent. And again the animal recovers his accomodation by such a change. That is, as far as the special dissatisfactory, displeased or painful state, associated with (or expected by) a certain cause (or motive), is concerned the desire to annihilate such dissatisfaction will have been satisfied by attaining the object through behaviours meeting this desire. Such a desire will, if repeated be strengthened to a cettain limit and afterwards be retained against its natural tendency to be forgotten. Accordingly knowledge with respect to the contents of this desire will be strengthened and remembered. But if the conduct to satisfy this desire has, contrary to his expectation, come to nothing or proved abortive, such a desire will be abandoned. Accordingly knowledge pertaining to such a "come-to naught" desire will be finally forgotten. Therefore, even at the sight of what once caused such a desire, the animal will remain indifferent.

By the way, referring to the above mentioned, we can infer the following: When we give only a small quantity of meat powder in order to make the conditioned stimulus, the sound of a flute, positive, as in Pavlov's experiments, the intensity of appetitive reflex stimulated by the sound (of a flute) will be suppressed and cannot go beyond a certain limit, because eating a small quantity of meat in a starving state will, on the contrary, deepen the sympathetic starvation and seccessively fatigue after the meal. Namely the dog wants to some degree, but gives up such a desire as goes beyond the degree.



Next, on the occasion of the conditioned stimulus, the sound of a flute, will strengthened and non-strengthed alternatively; firstly, the conditioned appetitive reflex, and also the conditioned sympathetic "inhibiting to eat" reflex formed by being given meat and coming to naught respectively, will be strengthened by a step-by-step repetition of the experiemnts, with rising and falling waves as shown in Fig. 7; and next, the acquiring reflex will restore a "negative", fatigued state caused by a strong conditioned sympathetic displeasing-hesitation reflex which lowers the quantity of flowing blood, oxygen, blood-glucose, etc. through the brain, nervous centers, etc., and also lowers the faculty of renewal in them, and the awakening hesitation-to-eat reflex will properly lose some of its intensity. This is a "waiting reflex" (2,p.112) Even if the flute is blown, meat will never be given. Namely the sound of flute is necessary but not sufficient for the meat to be given. In other words it is a non-essential conditioned stimulus (or sign) for a meal. And likewise the other external and internal conditions are almost invariably present before, during and after the strengthening-with-meat experiment are all non-essential stimuli. Such stimuli will excite, though slightly, the dog's appetitive and inhibiting nervous centers; and following this, he will, taking a look at meat, firstly excite his appetite strongly.---- However we infer that if the stimulus is seldom strengthened, (reinforced), the dog will almost always remain in a state of sleeping and will become indifferent to the stimuli; on the contrary, when the sound of a flute is almost always accompanied by meat, his external appetitive motion will become more brisk and his awakening intensity will be increased.

Similarly, on the occasion of the conditioned stimulus, the sound of a flute, being strengthened with meat and acid alternately, the conditioned appetitive and rejecting acid motions which suppress with each other will cause the dog to fall into sleep, lest the dog should fall into an over-fatugued state of its brain due to the remarkable sympathetic-displeasing strain. Or self-condradicting desires with self-colliding contents will be fitly abandoned so that the dog may not suffer from headache due to a difficulty in solving the contradiction. Namely it becomes a waiting reflex towards the conditioned stimulus which is sure to be followed by either meat or acid, although it is not sure which of the two is given. Moreover on the occasion of meat or acid being not given necessarily or of nothing being given at times, the dog will, of course, becomes more indifferent. For example, senses of feeling laboratory and corporal condition are included in the category of such conditioned stimuli.----Thus a "unconcerned" stimulus or a"new" one named by Pavlov as such, is such a conditioned stimulus as, being different from, for instance, sniffing or seeing meat, has no distinct conditioned reflex which can be refined as an appetitive or sexual desire reflex. In other words, this is a conditioned stimulus like numerous and various special reflexes, accordingly each-other suppressing reflexes which put the dog in a state of sleep to some degree through over-fatigue of its brain. In the contents of "attention" or "researching" reflex, called by Pavlov as such, are included such conditioned as well as unconditioned reflexes. According to the dog's living experience, it contains, on one occasion, sympathetic displeasure or pain, namely uneasy or horror, on another occasion, parasympathetic pleasure, satiffaction or curiosity and interest.

3. Specialization (or Discrimination) of Con ditioned Stimuli

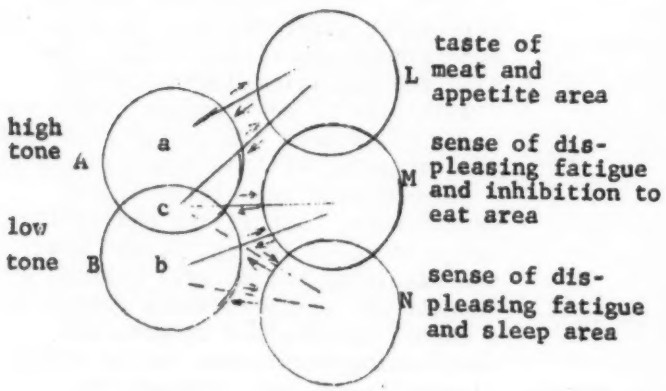
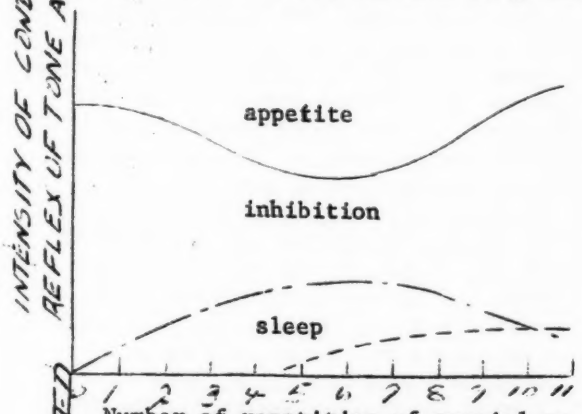


Fig. 8.

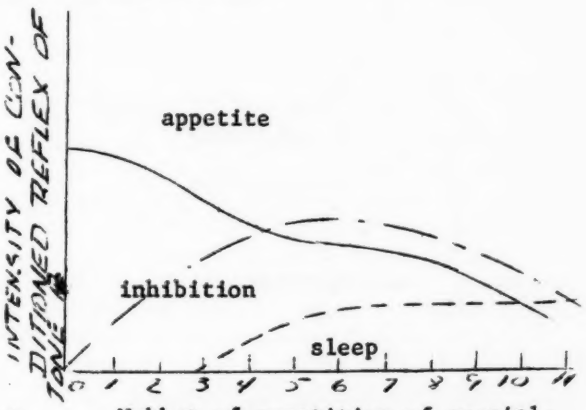
In Fig. 8. the repetition of conditioned experiments of blowing a flute of high (pitch) A accompanied with meat, will make strong circuits between auditory sense area A (a+c) for high tone and gustatory sense a area L for meat with the appetitive reflex in the cortex cerebri. And next if another flute of low tone having corresponding auditory sense area B (b+c) in the cortex cerebri is blown, the ratio between the intensity of conditioned appetitive reflexes stimulated by tone A and B is approximately (a+c) to c. Now, on the occasion of B, no meat is given. Due to the over-fatigue of the brain caused by the appetitive reflex which ended in vain, the dog will firstly get a food-inhibiting (inhibiting -to-eat) reflex. Namely conditioned circuits will be formed between auditory sense area B (b+c) and the sense area for fatigue of brain M with the inhibiting-to-eat reflex. Next test high tone A. In the proportion in which area c, common area to A and B, has come to inhibit eating, the conditioned appetitive reflex stimulated by A will be weakened. Then strengthen it again with meat. If we repeat such alternating experiments, both conditioned stimuli A dnd B will, step by step, strengthen their hesitation reflexes with sympathetic strain, accordingly increase the fatigue of sleeping nervous center. If the difference between both tones is very slight, both tones will acquire finally the conditioned sleeping reflexes and become waiting reflexes. (The dog falls into a state where it is impossible for him to discriminate them.)

If the difference between both tones is high beyond a certain degree, the conditioned stimulus B tone, having sympathetic strain-reflex being stronger than that by A with ratio (b+c) to c, will firstly acquire a sleeping reflex; and conditioned circuits will be formed between B and sleeping area with the sense of fatigued brain L in the cortex cerebri. In proportion as tone B has been slept to, next the conditioned reflex with sympathetic strain stimulated by tone A will be lowered, restoring fatigue by means of the following, and para-sympathetic pleasing meat will be made easy and the conditioned appetitive reflex, lowered once, will begin to be restored to its first intensity. Thus, according to their repetition as shown in Fig. 9. on one hand, the conditioned reflex of tone B becomes nearer to nothing or indifference, on the other hand, that of tone A will be recovered to its former intensity. Namely the conditioned appetitive reflex is concentrated and strengthened in a part of area A, a, as shown in Fig. 10.----



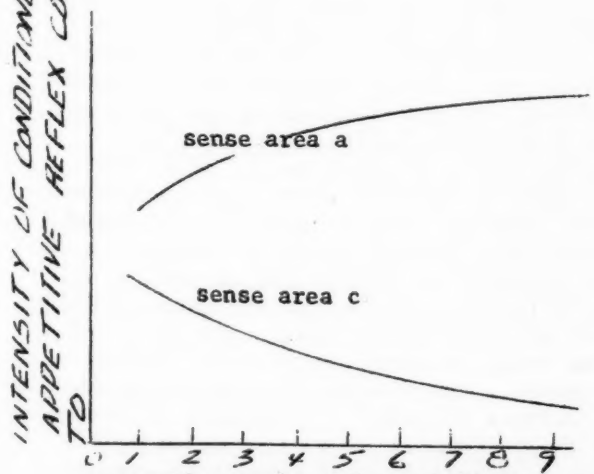
Number of repetition of specializing experiments

Fig. 9.



Number of repetition of specializing experiments

By the way, to connect conditionally high tone A with the taste of meat means, generally speaking, to change the uneasy, searching (or watching) reflex, which A had already, into an appetitive one. Therefore, tone B has still the watching reflex considerably. Consequently this "new conditioned stimulus" so to speak has often its appetitive reflex remarkably inhibited, being different from Fig. 9. by this watching one in the beginning of those specializing experiments. (1, Lecture 7.)



repeating number of specializing experiments

Fig. 10.

Moreover, we assume that the difference between tone A and B to be specialized lies not in that of situation and dimension of the excited part of sense area as shown in Fig. 8., but in that of the distribution of excited intensity in the same part of the sense area. For example, let us assume, premising Helmholz's theory, auditory sense cells are arranged continuously (or linearly as along LM in Fig. 11) corresponding to their vibration number; and that the distribution of the excited intnesity of auditory cells stimulated by sounds A and B are shown as curves LAM and LBM respectively. On this occasion, thinking briefly the intensity of the conditioned connection between the sense area for the taste of meat with the appetitive reflex and auditory cells is to be distributed principally as LPM Corresponding to LAM; and that between the sense area of displeased fatigue with

the inhibiting-to-eat or sleeping reflex and auditory cells is to be distributed as LQM corresponding to LBM. By this too, it's the same as in the occasion shown in Fig. 8 to reflect appetite by stimulus A and inhibition for eating by B.---- The mechanism of specialization in the sense area is generally unification of both cases as in Fig. 8 and Fig. 11.

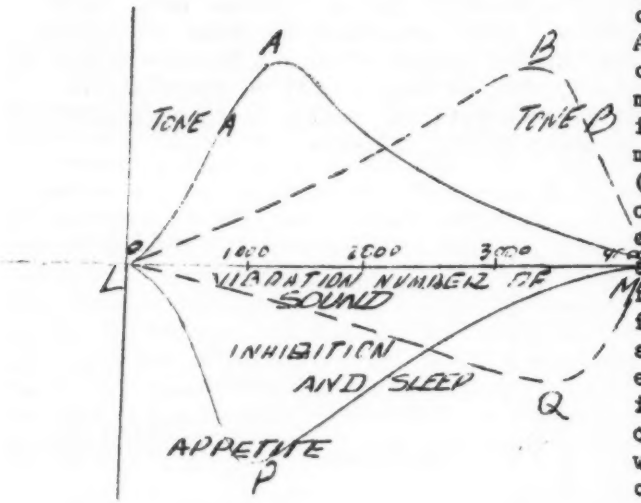


FIG. 11.

The abstracted relation between special conditioned stimuli A and B can be shown by the excited sense areas A (a+c) and B)b+c) in the cortex cerebri. In a special case, a or b may be lacking, namely the one may be included in the other (or the other may be the one plus a new stimulus) (1, Lecture 5). The specialization of intensity is one of the tangible examples. If the difference of excited sense area corresponding to the difference of the sound intensity of flute corresponds to the increase of such a distribution curve of the excited intensity as shown in Fig. 11, in proportion to the sound intensity of the flute, direct specialization will not be made, but generally it corresponds to that of the situation and dimension of the excited sense area as shown in Fig. 8, so a direct specialization is possible. By the way whether specialization can be made or not is related to the ratio between the common sense area "c" and the non-common area a or b. Accordingly if the excited domain is to be increased principally in proportion to the sound intensity the minumum value of the difference of the sound intensity which can be discriminated, namely the "discrimination threshold", is also to be increased in proportion to the increase of sound intensity. The is "Weber's Law". However the variation of a stimulating sound intensity changes necessarily the intensity of unconditioned reflexes, for instance a researching reflex, accordingly its quality and therefore, as a matter of course, the specialization of different intensities is possible by making these senses of corporal state into additional conditioned stimuli. Moreover Pavlov said "time can also be made a conditioned stimulus." (1, Lecture 3) Originally "time" is a concept which is gotten by abandoning their tangible properties from concepts of "change and motion". Accordingly with the elapse of time, outer-circumstances and so the sense for them will change, mediated by unconditioned and conditioned reflexes. And if a corporal condition changes, the subjective sense oo outer-action will, though remaining the same as an objective one, change. Accordingly the dog can specialize those changes of stimulation. Pavlov's "delayed stimulus" (1, Lecture 6) "trace stimulus" (1, Lecture 3) etc. are one of those examples.----Next concerning a problem with respect to the specialization or elimination of stimuli of circumstances and corporal conditions which are inseparably attached to the positive abstracted stimulus, the sound of a flute, attention must be paid to the fact that, firstly that the separability between stimuli of outer-circumstances and inner-corporal condition is relative; and secondly, the elimination (or to make sleep) of those non-essential stimuli as having taste if meat with appetitive reflex is different from naught, accordingly those stimuli have, though slightly, appetitive and hesitative reflexes.

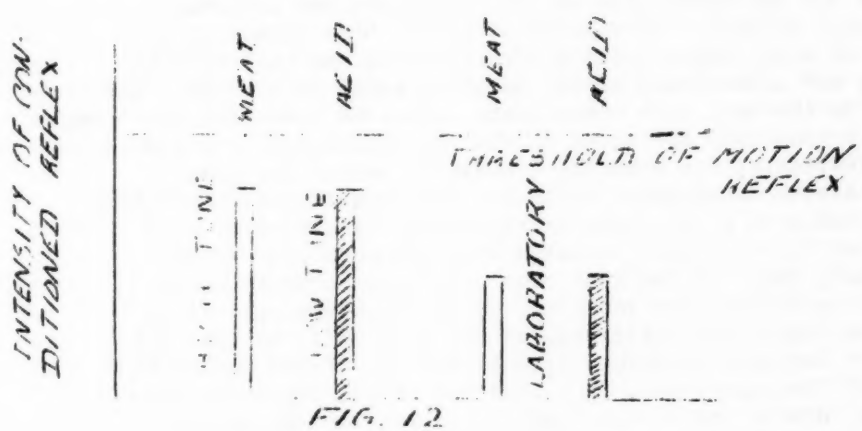


FIG. 12

What has been mentioned above holds true in the specialization of experiments, that is, in the strengthement of a high tone with meat and a low tone with acid. After having acquired conditioned reflexes, non-essential stimuli, sense of circumstances and corporal condition come to have so much sleep-appetitive and reject to-acid reflexes as not to be able to actualize themselves, often-times as not to be concious of themselves, by the stimulation of themselves only. For example, as in Fig. 12, the sense pertaining to the laboratory can, by these stimuli, only reflect neither appetive-for-meat nor rejective-for-acid motions; but, with the addition of a flute-sound, it can reflect either the appetitive or rejective motion in accordance with whether it is a high or low tone. Namely, speaking concretely, stimuli pertaining to the laboratory come to reflect different reflexes in accordance with the difference of separable of added stimuli, of the sound of a flute and so on. By the way, separability has inseparable relations with inseparability. So if we attach great importance to the latter, and regard those instruments, flute, etc. as indispensable on the laboratory, the dog, it can be said, comes to reflect differently, discriminating the changes or difference of the contents of the laboratory. Similarly, in Fig.12, if we take the dog out of the laboratory and let him hear the flute, he will not reflect any appetitive motion. Thus, an abstracted stimulus, for instance the sound of a flute, must be grasped as having different conditioned reflexes an accordance with the difference of the sense of circumstances and the corporal conditions under which it is played. For example, the same apple becomes an object of different desires according to whether one is in a state of hunger or satisfaction, to where one is,---in a dining room, a studio, a botany class-room, a chemical laboratory or so on. Namely "conditioned reflexes", (knowledge, concepts, etc.) are always formed, discriminating the speciality of the whole sitmuli (or of the whole sensation given.)

4. Essential Conditionid Stimulus

Bring the dog into the laboratory, let him hear the flute of high tone followed by meat. (Refer to Fig. 12) Now let us take him out of the laboratory, for instance, into a auditorium, on a street and so on, and let him hear the high tone. He will not secrete saliva. The high tone is not the abstracted essent al stimulus to reflexive appetite for this dog. Namely it does not necessarily follow that, if the flute of a high tone is blown, appetite will always be reflected, irrespective of the circumstances and corporal conditions. In order to make this abstracted stimulus essential for reflexing appetite, this high tone must be strengtheened with meat in the auditorium, on the street and so on.---After such experiments have been carried out, conditioned connection between

the taste of meat area and the sense area for circumstances and corporal conditions, non-essential stimuli. Consequently, after that, if he takes a glance at or sniffs at meat, under certain circumstances, he will come to recollect abstractedly and selectively of the auditory sense of a flute. And this is an "Image."---By the way, such experiments cannot be completed in a single day. Accordingly, it's generally different to make an abstracted conditioned stimulus essential. Therefore, for example, immediately after the sound of flute was made appositive conditioned stimulus, its secreting-saliva-reflex would be easily inhibited even by a slight rearrangemant of the room, by a slight odor or the noise by a stranger entering into the room and so on. (1, Lecture 4) Similarly when the low tone, the positive, even by slight change of his corporal condition, for instance, by making him sleepy or by a little change or room light, the negatively of low tone would be lost and saliva be secreted by a low tone stimulus. (1, Letture 7) Viewed externally, it is the phenomenon of "Degeneration". (2,p.134) One of the important moments (or objects) of "habit, discipline or exercise" is to make an abstracted conditioned stimulus an essential one for a special reflex. The abliltiy of a champion who has trained himself well will not be affected even by a considerable change of his corporal condition or by a change of place where the play is to be held. And those matters mean that a habit or prospersity once made, good or bad, will not be changed easily.

However, the domain in which an abstracted conditioned stimulus is so essential as to elecit a certain special reflex would be limited so as to accommodate itself to circumstances, even though one had plenty of time to gain sufficient experience. For example, in front of a tiger, a dog may acquire a horrow reflex, but not acquire, nor accordingly regenerate, an appetitive reflex for meat stimulus which, but for a tiger, would be very nice for him. The contents of conditioned reflexes which are high as "Will or Cognition" and require one's high attention or strain to get them cannot be acquired, accordingly re-generated, in an over-fatigued state or in a sleepy condition. And this is one of the reasons why dreams are primitive, instinctive and infantily.----Reversely speaking, the contents of conditioned reflexes acquired, and so regenerated, under certain consitions, namely under certain circumstances and in a special corporal condition, are limited according to the speciality of their condition. For example, all that attracted one's attention in a dining room, not to speak of an apply, is liable to be seen, or talked about, as an object of appetite or what is related to it.

The next problem is related with what is essential as a special conditioned stimulus. When we use a dog bound with suspending bands as in Pavlov's experiments and make him discriminate a circle figure from a squre one thrown upon a screen in front of him, as those images made of the retina of the dog gazing at those figures, are invariable, it is possible for him to discriminate them directly, by making those images as conditioned stimuli. But when the dog is set free, those images made on his retina, will be changed according to distance between the screen and him. Accordingly there can hardly exist such essential eye-sight cells as are excited always on the occasion of circle figures but never excited on the occasion of square ones, irrespective of the distance between the screen and him. Namely it is difficult to have him discriminate them directly by making those images or on his retina as conditioned stimuli. Such direct eye-sight stimuli are non-essential as special conditioned stimuli. The sense area corresponding to such an image on his retina has a wea special reflex with hesitation and sleeping to the circle, smooth movement of his eyes along the circumference and so relaxed posture accompanied with it, and therefore their sense, appear commonly and invariably in the event of the circle experiments without fail, but never on the occasion of square experiments. And it is radically different from the common

special researching motion pertaining to square figures, angular movement of his eyes along the square and so accompaning stiffened posture, accordingly their sense. The conditioned connection between these stimulating sense areas and the specials conditioned reflex area is especially strong. Namely, such are essential as special conditioned stimuli. Accordingly they have a characteristic of a "Gestalt". Namely a dog that has acquired a discrimination between a circle and a square of certain dimensions can also discriminate the former from the latter even if they are of different diminsions. It is generally difficult to discriminate size, colour, intensity of light, etc. by our eyes directly due to the fact that these direct senses are easily affected with the change of circumstances and our corporal conditions. And the same can be said in regard to other receptors. Consequently, an animal trained to chose the medium sized vessel of a pair of big and medium ones arranged side by side, will mostly chose a much smaller one if a pair consisting of a medium and a small one are now shown, especially in case the difference between them is small. Senses corresponding to cognition of the smaller, namely those of imitation movements accompanied mainly with excitement of bending muscles, such as an expiring and shrinking of animal's body, when the animal moves int eyes from the bigger to the smaller, has not relation with the distance between vessels and the animals, accordingly those senses become more essential as special conditioned stimuli. than those corresponding to direct the image of vessels formed on animal's retina. Similarly when a couple of different grey papers are used, senses corresponding to cognition of "the darker", namely those of the unconditioned reflex to open its eyes wide, have no relation with, for instance, the light intensity of the room, his corporal consition, etc. (3,p.200) The reason why, in physics, we don't adopt the method by direct perception which is conspicuously affected by the change of circumstances and corporal conditions and is said to be "subjective", but the"length comparing" method, lies in the abonve-mentioned.---And these essential conditioned reflexes which are easy to acquire and so strong, are, on the other hand, what will be remembered for a long time, accordingly diffecult to be forgotten. By the way, what is said to be "special" or "characteristic" of an individual object is tantamout to such an abstracted essential special conditioned stimulus. Accordingly the more time elapsed, the more the circumstanes and corporal conditions change, the more non-essential parts will be lost, and the more the speciality of contents of regenerated memory as their general tandency is exaggerated. (3, p.289) When we cannot recognise who hi is, even by the sight of his back or his front under his neck, we shall be able to indentify him if ee take a glance at thi face, or even only at the mole on his nose, his special feature. Namely the difficulty to regenerate conditioned reflexes does not dependNupon the dimension of restimulated parts but upon whether essential stimuli are included in them or not. (3, p.291) And images are such special features abstracted or exaggerated.

In conclusion, in Fig. 13, if discrimination between high tone A, positive, and low tone B, negative, has been completed, then that between A and middle tone D, negative, will becnme easy. Because the conditioned appetitive reflex by D will be weak due to c, contained in co mon area with A, having already been extinquished ; accordingly, fatigue produced as reaction in the Cortex cerebri and so the inhibiting reflex acquired will be weak. Accordingly the sympathetic hesitation reflex by the next stimulus A, as well as due to the concentration of the conditioned-appetitive-reflex passages, will be made weak and so tone A will be easily strengthened b meat without the dog's falling into a fatigued state of brain. That is, if a coarse discrimination has been formed, then the

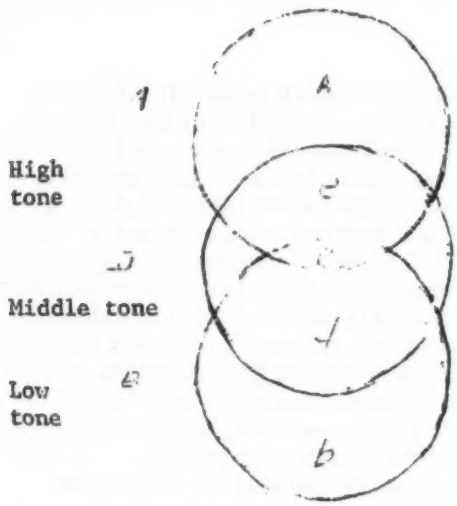


Fig. 13

more minute discrimination will be easily formed. Thus, if we make the discrimination more minute step by step, the minute discrimination which could not be formed without such a process will be finally formed. (2, p.135-138) Moreover a similar conclusion will be derived from the premise as shown in Fig. 11. And this is one of the most important formule with respect to teaching, learning and so on. And it is one of moments or objects of forming "habits", learning" or so on to increase adaptability by making discriminations more minute, as well as acquiring and strengthening of conditioned reflexes and making abstracted stimuli essential to elicit special reflexes as already mentioned. For example, an excellent swordsman percieves his enemy's next move by a slight move of the eyes, or of the tip of the sword of his enemy.

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(3) Kohler W. (1928): Gestalt psychology. Trans, by K. Sakuma. Tokyo. Uchidasokakuho (1944)

THE WORM'S-EYE POINT OF VIEW

FROM: Richard H. Linn, M. D.
Chief, Medical Service
U.S. Public Health Service Hospital
San Francisco 18, California

I have recently found a reference to and quotation from your DIGEST. The latter leads me to believe we at this hospital might derive great benefit from the regular perusal of your publication. Please tell me what the subscription price is in order that I may inform the medical librarian and have her submit a recommendation for its subscription to the Library Committee. If there is no price for the subscription and you are willing to donate one that yould be fine too; however, if you can send but one, please address it to me. After I have perused it from cover to cover I will be glad to place it in the library.

I would be interested to know if others than your immediate group can contribute to the DIGEST. If such contributions are acceptable, there are a few members of my own intimate group of friends who might make worthy correspondents.

---(There is, of course, no monetary charge for the DIGEST--any subscription money sent us is either returned or spent on beer. We are most anxious to receive scientific contributions, anecdotes, preliminary research reports, droll tales, etc. We pay the lowest rates per word in the field, but you get to see your name in print.--Ed.)

FROM: T. J. C. Berk, Ph. D.
Netherlands Central Institute for Brain Research
59B Mauritskade, Amsterdam

Working in the field of conditioning and being very interested in such critical experiments as you did (but I haven't the exact facts) I would ask you if you can help me to the publications of the WORM RUNNER'S DIGEST, in which the publications I heard of were published.

I am starting this kind of work here in Holland and am working with Dr. Schade here. I would be very grateful if you could send me material and studies as they are not available here.

FROM: G. W. Jackson
503 Bradford Road
Oreland, Pennsylvania

Herewith a picture of a Columbian earthworm, taken from the Cleveland (Ohio) PLAIN DEALER of June 13th. This is a tremendous worm - the biggest damned worm you ever saw. This is the grandfather of all worms.*

Also herewith an item, which embodies all the good qualities of worms everywhere. Unfortunately I cannot locate its original published source.**

--(*The cutline for the picture reads: "New York-- Mrs. Marte Latham holds her Colombian earthworm, Gertrude, at her apartment toshow that it can expand to five and a half feet and then return to its normal two-foot length. Gertrude was shipped to London by airliner in an ice-chilled crate for scrutiny by worm experts at the London Zoo. Mrs. Latham, an explorer, discovered Gertrude in the Andes Mountains of Colombia."

**Walter Sykes of Bethesda, Maryland, writes: "Earthworms never cause anyone any trouble. They require no special care or feeding, and they never wake you up in the middle of the night by barking and howling. Earthworms never get rabies, mange, measles, neurosis, athlete's foot or paranois, and, consequently, they never run up large medical bills. I have been bitten several times by dogs and small boys, but never by earthworms. Earthworms are never disloyal to their friends (they don't have any) or to their masters. In fact, many of them are more loyal than some dogs (not every dog would give its life just to get its master a fish). Furthermore, earthworms do not engage in crime, wars or slander, and no earthworm has ever been known to assassinate a president. So, you see, earthworms are better than both dogs and people.)

FROM: W. V. Macfarlane, M.D.
The John Curtin School of Medical Research
The Australian National University
Department of Physiology
Canberra, Australia

It was a pleasant surprise to receive the Wormrunner volume III number 1. Thank you for the gesture. The 75 pages were snatched from our hands by eager biophysicists and physical biochemists who wished to read some of the original prose that led to Koestler's story in the OBSERVER. I have now been able to read it. It is good to have your refreshingly direct approaches to learning processes. This synapses with some of our studies on the spinal end of spinal cats. We have been able to stop the reflex movement of hind legs by repeated touch, hot or cold stimuli. The habituation is slow (20 days) initially and the forgetting or return to activity when not stimulated takes only 10-20 days. Repetition of the stimuli (6 minutes once a day for instance), brings on the inhibition again in 8-10 days; while on the third time of learning, reflex movement ceases after 2-3 days. When cold is applied daily for a month, there is no forgetting in 3 months, on testing weekly. The spinal cord can habituate to cold while forgetting about heat. The learning (if the term is elastic enough to take this meaning) can spread from the left leg to the untrained right leg in the course of a month.

All this could be explained by the action of interneurone fields creating a presynaptic inhibition of afferent input on its way to the motor neurones. When stimuli are applied for 0.004 day, there is some residual effect after 24 hours. The interesting questions, of course, is what sort of chemical change produces the prolonged effect. These situations are simpler than conditioned responses, and should be reasonably available for analysis.

The neurophysiologists and biophysicists round here are anticipating more worm lore, and so am I.

FROM: Michael Strasser
Junior High School 135
Bronx, New York

I thought it would please you to know that I placed a first at the New York City Science Fair last Saturday, April 15th, with my entry called "Conditioned Reflexes in the Planaria." The judges seemed to be interested in this kind of work in general and were interested in the research group.

I used the conditioning methods with massed trials as described on page 34, Vol. II, No. 2 (of the DIGEST) with astounding success and my regeneration experiments too were successful.

I plan to continue doing work with planaria, and this summer I will start work on some experiments in comparative psychology of Learning using various kinds of mazes. I will write to you from time to time of my progress.

FROM: Alexander Kohn, Ph.D.
Department of Biophysics
The Israel Institute for Biological Research
Ness-Ziona, Israel

Intelligence has reached the Secretariat of the Society for Basic Irreproducible Research that you publish the WORM RUNNER'S DIGEST. We should be very much interested to have our Society on the mailing list of your Digest.

Instead of paying the subscription dues (if there are any) for your Digest, we offer in exchange our publication, the JOURNAL OF IRREPRODUCIBLE RESEARCH, which appears 3 times a year.

---(Hold on there, Dr. Kohn. We may have an article for your JOURNAL any day now!--Ed.)

FROM: Jack Denaro
281 Ribbon Street
Franklin Square, New York

Enclosed find a manuscript containing a brief summary of experiments that I did concerning the acceleration and retardation of the regeneration rate of Planaria. I sincerely hope that it is of sufficient interest to the subscribers of the WORM RUNNER'S DIGEST to warrant its publication.*

If I may mention it, I entered this project in the H. Frank Carey High School Science Fair, where it won, much to my surprise, first place in the chemistry division, and best of fair.

---(We did find it of sufficient interest; see elsewhere in this issue.--Ed.)

FROM: John Paget
The Travellers
25 Avenue des Champs
Elysees, Paris, France

A great deal of science is being devoted to breeding flat worms and very little to the production of racehorses; may I suggest then that you add a Horse-Runner's supplement to your distinguished publication?

The ears of equids seem to vary in size and mobility in inverse proportion to their speed. This is particularly true in the cases of the various breeds of Zebras, Asses and Onagers; the Shetland pony, however, would appear to be the exception to prove the rule; for, if it has mean little ears, it has never needed larger ones, since the fauna of its natural habitat does not include carnivorous animals, with the exception of man, who has never bothered to stalk equids finding it so much easier to drive them.

It has proved surprisingly easy to breed back to the original wild proto-type from domestic animals like cattle; but in the case of the latter the Zoologists knew exactly what the auroch, the proto-type they were trying to reproduce, looked like, and were, in consequence, able to breed back by selection; and nature was obviously on their side in this endeavor.

In the case of the racehorse this is precisely the process that must be avoided at all costs; but since breeders have not been at sufficient pains to discover the true origin and nature of their foundation stock, they have no idea what is likely to provoke a disasterous reversion to type. For example, all things being equal, no one would hesitate to mate two fast animals with exceptionally long and mobile ears, although such a course would appear on the face of it absolute folly.

Then again there is the neglected possibility of breeding a more specialized racehorse, that would enjoy a physical advantage over specimens of the breed, as we know it today, particularly in America where most of the tracks are very much alike.

It has just come to light that such a phenomenon was running in England about ten years ago and proved far and away the best horse of its generation at all distances up to a mile, and a mile in England represents some ten furlongs under American conditions.

The peculiarity about this animal was an obstinate refusal, shared I am creditably informed by the dam, to lead on the near-fore--that is to say they both refused to gallop in the manner that comes easiest to domestic and feral horses, with the off fore (right front leg) and near hind legs in support, the other diagonal cutting out the work.

On a right hand track such a horse would have enjoyed much the same advantage as the world record holder, Barry Halberg,

running indoors and turning towards his withered arm and shoulder; for it stands to reason that a normal horse that has been trained to use both diagonals indifferently carries an unnecessary load of muscle that its heart has to supply.

Considering that the horse under discussion, Tudor Minstrel, won our first classic over a straight mile in a canter, the ability of other runners to change legs (diagonals) at the expense of half a length a time, was obviously a very inadequate compensation.

The racing fraternity, however, is as orthodox as the faculties of biology at universities; so the trainer, who felt that Tudor Minstrel's idiosyncracy reflected upon his professional ability, kept his own counsel, and it was only by the purest accident of chance, that, after his death, the jockey, Sir Gordon Richards, disclosed this secret to the racing correspondent of HORSE AND HOUND.

An ex-member of the faculty of biology of Harvard has informed me that foals dropped on the Herrera ranch in Peru by dams of Spanish origin, that still breed true to type, amble off after the mare, as if to the manner born, although pacing, as opposed to trotting, is not a natural pace of the horse.

If an artificial action, originally imposed on this breed by the use of hopples, has in fact become transmissible, it would seem to prove Lysenko's theory up to the hilt, and it encourages us to believe that specialized racehorses, that can be relied upon to go on one diagonal exclusively, could easily be bred. From the foregoing it will, I hope, be clear to worm-runners that there is much more future in inducing useful conditioned reflexes in racehorses than in Tigrinae.

---(We heartily approve of Mr. Paget's Horse Runner's Supplement to the DIGEST and hope we can prevail upon him to continue to supply us with same. In particular, he has promised us a "Zoological report on the first domestic horse so far discovered. It was found at Bahen in the Sudan and the skeleton is in very good repair considering that it is nearly 4000 years old." We also approve of the idea of establishing conditioned responses in racehorses, particularly the kind of CR that pays off at the pari-mutuel window!--Ed.)

3

THE WORM RUNNER'S DIGEST Vol. III #3

An Informal Journal of Comparative Psychology

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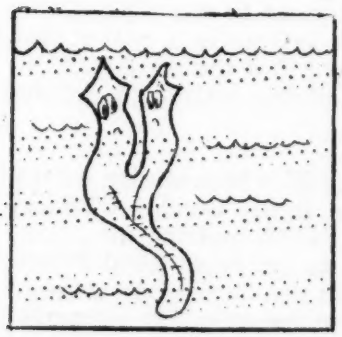
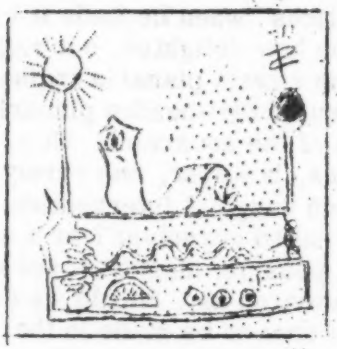
WORMS AND THINGS

Scientists are supposed to be objective and rational; worm runners are supposed to be scientists. Therefore, as any clinical psychologist could deduce, worm runners (like all other scientists) may be expected occasionally to demonstrate that very human and very irrational form of behavior which Fred Skinner has called "superstitious" when he finds it in his pigeons. With this in mind, you can imagine how delighted, yet vaguely uneasy we were when DONNA STONE, one of the newest planaria trainers here at the Britannica Center, discovered a genuine two-headed planarian creeping its duo-cephalic way along the bottom of the aquarium. Now, there is nothing unusual about two-headed worms, however, one rarely finds them occurring naturally without any known surgical intervention. Was this the result of a splitting headache, we asked ourselves? or a new form of animal schizophrenia? The number of possible diagnoses seemed endless, but it was the prognoses that really bothered us. Should we see this as an omen, a portent of great discoveries soon to be made in the platyhelminthetic realm, or as an augur of impending schismatic doom? Does the safety of science lie in wooden walls or in bifurcated flatworms? Perhaps only the Oracle at Delphi could handle that sort of question.



In light of the above, and believing firmly that the gods help those who help themselves, we are pleased to report that the Planaria Research Group is not only surviving, but even prospering in its own presently twin-prowed state. Here in sun-drenched California, ALLAN JACOBSON is coming along nicely with his attempts to demonstrate the cannibalistic transfer of training in a maze situation. REEVA JACOBSON is working on the "third re-generation" study using mazes instead of classical conditioning, and DONNA is helping both of them. Meanwhile back at Michigan's snow-drenched 40 acres, PAUL and GRETCHEN CORNWELL are spending their holidays running "winter worms" to compare with the worms they ran last spring and summer, RAY KAUFFMAN is completing the various radiation studies we've had going for the Atomic Energy Commission, HAL SALIVE is working on maze learning, MARGARET CLAY HARRY DOERR and PAUL GALENTINE are investigating the planarian's nervous system by means of electron microscope (with the help of a grant from the National Institute of Mental Health), while BOB TREBILCOCK and DAVE BOUSE provide generalized but important assistance. MARGARET CLAY is in general charge of the Michigan labs. Progress reports are promised for the next issue of the DIGEST. We do not promise any data on the speed with which a two-headed flatworm learns to go left in a T-maze, however, for we haven't the heart to run such a study.

* Some of our readers must wonder, from time to time, if worm runners in general and the PRG in particular aren' t more interested in their publicity than in their planaria. HARLAN LANE has probably stated our viewpoint nobly with his cover poem, "Empty Planaria Do Not Make the Most Noise." The problems involved in publication versus scientific "purity" are thoughtfully outlined in Reif' s "The Competitive World of the Pure Scientist" published in the 15 December 1961 issue of our sister publication SCIENCE. Hopefully, those who do good work get the most public notice, although there are certainly exceptions. We hope the PRG is not one of these exceptions.



Whatever the case, this has been a particularly good time of year for public notice of worms. PRISCILLA GINSBERG did a most excellent summary of our cannibal studies in the 11 September issue of NEWSWEEK. That worthy publication also came up with a clever story on BOB SOMMER' s "Second Law" (on Keys and Status) straight from the summer issue of the DIGEST. The grand, gray NEW YORK TIMES did likewise, but being the most non-frivolous publication in the world, refused to reference the story as coming from the WORM RUNNER' S DIGEST preferring, instead, to say it had originated in the PRG' s "Informal Journal." We were also pleased to see STANTON DELAPLANE devoted a full column to BOB' s article in a recent issue of the SAN FRANCISCO CHRONICLE, and to learn that the AMERICAN BEHAVIORAL SCIENTIST printed a summary of the article (and listed its source correctly).

On a different note, ARTHUR KOESTLER' s beautifully written exposition of our work first published last spring in the London OBSERVER was reprinted this October in the Washington POST, netting us a flood of inquiries from the Nation' s Capital (but none as yet from 1600 Pennsylvania Avenue). LOUIS PANUSCH' s article in the November issue of the SCIENCE TEACHER has yielded us, to date, more than 100 responses from high school students and their teachers. The nation' s Science Fairs should be crawling with worms this year.

The truth of the matter is that we have received such an unexpected and overwhelming response to recent issues of the DIGEST and to stories like KOESTLER' s and PANUSCH' s that we have often fallen considerably behind in answering both requests for back issues of the DIGEST and for information on the care and running of worms. To all of our new readers who sat for weeks impatiently waiting their copies, we apologize. Much of the blame for our success, however, must be lodged at the door of one ROBERT BEYERS, who, while working in the News Bureau at The University of Michigan, was most instrumental in seeing that the DIGEST and the worms received more than their due of publicity. BEYERS has since moved here to Palo Alto himself to head the News Bureau at Stanford and threatens to continue his kind efforts in our behalf. We wish him luck.

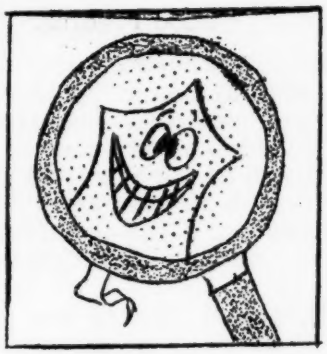
The tremendously exciting work by BILL CORNING and E. ROY JOHN on the effects of RNASE on the retention of a conditioned response in regenerating planaria was reported formally in SCIENCE late this fall (Vol. 134, pages 1363-65) and received a well-justified commentary in the "Science and the Citizen" column of the December issue of the SCIENTIFIC AMERICAN. We add proudly that DIGEST readers learned of this fascinating work more than a year ago.

And, to round out things on an international note, we are in pleasant receipt of communications from MASAHARU KAWAKATSU and from A. ICHIKAWA of the Zoological Institute of Hokkaido University enclosing a copy of an article by Professor ICHIKAWA taken from the Japanese journal, THE HEREDITY, on "Planarian Regeneration" which begins with a description of the DIGEST. Also enclosed was a photostate of an article on our experiments taken from a February issue of the Kyoto Shinbun newspaper.

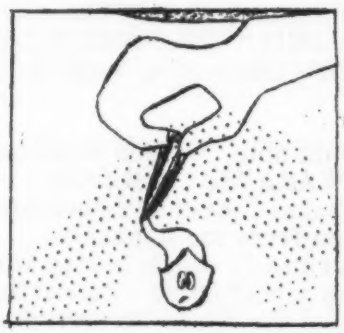
*Recently we received some experimental results from JOHN H. HOLLIS, of Yerkes Laboratories, concerning his continuing studies with mealworms which we herewith pass along. HOLLIS has apparently been able to establish associative hysteresis in his animals and reports that the "curve for tactile stimulation is similar to that obtained for the flatworm." Shades of Hovey! HOLLIS also reports some interesting findings on habituation in the mealworm, namely that when he habituated his animals to an electric shock, they still responded vigorously to tactile stimulation. This gives additional evidence that habituation is, at worst, a very primitive but quite specific form of learning.

*CARL C. LINDEGREN, who is director of the Biological Laboratories at Southern Illinois University, was kind enough to send to us a first draft of his book, THE COLD WAR IN BIOLOGY, which was outlined in the DIGEST last year. We found it a most provocative, challenging and worthy piece of thinking, recommend it highly, and will inform our readers as soon as possible what the book's publication date will be.

*IN THIS ISSUE... JOEL COHEN is one of those frightening young geniuses who appear on the scientific horizon all too seldom. At 15 he wrote (and published) a treatise on information theory and the structure of music and has, we believe, a major monograph on this subject (written at 17) coming out soon. Also, when barely 17, he translated a book on information theory from French into English for a leading American published. Now a hoary and ancient 18 or so, he's a sophomore at Harvard (having skipped the freshman year there by taking a few exams) and is studying mathematics. When not being creatively mathematical, he writes music. When not doing either, he runs worms. JOEL is particularly interested in the effects of temperature variations on learning speed



in earthworms and we hope to persuade him to publish some of his results in a later issue of the DIGEST... JAMES E. MILLER' s outrageously funny and perspicacious account of how Newton discovered the Law of Gravitation was called to our attention by Mrs. LURA JACKSON, who is Chief of Publications and Reports for the National Institute of Mental Health, and we are as much in her debt for so doing as we are in MILLER' s for having had the insight (and talent) to turn out such an amazing exposé... BARBARA HUMPHRIES has given up (at least temporarily) the study of cannibalism in planaria in order to satisfy her own intellectual hungers, and is now a graduate student in philosophy (!) at Harvard... HARLAN LANE is a Skinner-oriented colleague of ours at Michigan who, like JOEL COHEN, shames us by his striking combination of youthfulness and wisdom. A Harvard Ph. D. at 22, an assistant professor at 23, HARLAN is particularly interested in verbal behavior in man and animals... For a while we feared that BOB SOMMER' s new job in the Psychology Department at the University of Alberta would be so demanding that he wouldn' t be represented in this issue (a groundless fear), but were consoled that BOB' s brother-in-law, MO TWENTE, a neophytic psychiatrist, and BOB' s Departmental Chairman at Alberta, JOSEPH R. ROYCE, had both sent along humorous manuscripts of their own. BOB tried to warn us against commerce with his wife' s relatives " and other nefarious individuals," but we decided to ignore his admonitions, as witness this issue... THOMAS MORRILL is soon off to Australia but promises the concluding instalments of his delightful ALICE before he goes. We expect to receive a manuscript on the " Nesting Behavior of Kangaroos" from TOM in the not too distant future... RODABE P. BHARUCHA-REID continues the discussion between her and JACK ARBIT concerning the behavior of earthworms in mazes in this issue. Dr. BHARUCHA-REID' s husband now teaches at Wayne State University... The " new look" in this issue of the DIGEST comes from the joint efforts of JUDY KNOWLTON, who is responsible for the splendid cartoons, and CAROL MILLER, our faithful amanuensis, who did the typing and lay-out... And we were " covered," as always, by REEVA JACOBSON.



* Good reading!

TITLE: HOW NEWTON DISCOVERED THE LAW OF GRAVITATION[1]

AUTHOR: James E. Miller, Chairman
Department of Meteorology and Oceanography
New York University

A tremendous increase in the number of vigorous young workers in the scientific vineyard has been one of the happiest results of the recent expansion, encouraged and nourished by our Federal Government, of scientific research in this country. These neophytes, left to their own devices by harassed research directors, have often found themselves without adequate guidance through the intricacies of governmental sponsorship; but, fortunately, they can find inspiration in the story of Sir Isaac Newton, his development of the law of gravitation, and his experience as director of the Subproject for Apples of the Fruit-Improvement Project, sponsored by His Majesty' s Government of Great Britain in co-operation with a syndicate of British fruit growers.

Few are familiar with the details of Newton' s twenty-year search for a proof of his hypothesis: the frustrations and failures, the need for accurate measurements of the earth' s radius and for a mathematical tool that Newton himself was forced to invent, and the integration of his scattered efforts by the splendid organization of the Fruit-Improvement-Project. These details have been collected from his Principia, personal letters, notebooks and other papers, and a series of personal letters, notebooks and other papers, and a series of personal interviews arranged by a medium of the author' s acquaintance.

In 1665 the young Newton became a professor of mathematics in the University of Cambridge, his alma mater. His devotion to his work and his capabilities as a teacher and friend of the student may be assumed without question. It is well to point out also that he was no dreamy, impractical inhabitant of an ivory tower. His services to his college went far beyond the mere act of classroom teaching. He was an able and active member of the college' s curriculum committee, the board of the college branch of the Young Noblemen' s Christian Association, the dean' s advisory committee on scholarships and awards, the committee for discipline, the grounds committee, the publications committee, the ad hoc committee, and numerous other committees essential to the proper administration of a college in the seventeenth century. An exhaustive compilation of Newton' s work along these lines reveals that, during a five-year period, he served on 379 committees, which investigated an aggregate of 7, 924 problems of campus life and solved 31 of them.

Newton the genius was yet a human being; and though, in energy and ability, he far surpassed the great majority of his fellow men, he found himself ultimately limited in his powers. His unselfish devotion to the important work of his committees absorbed so much time that he

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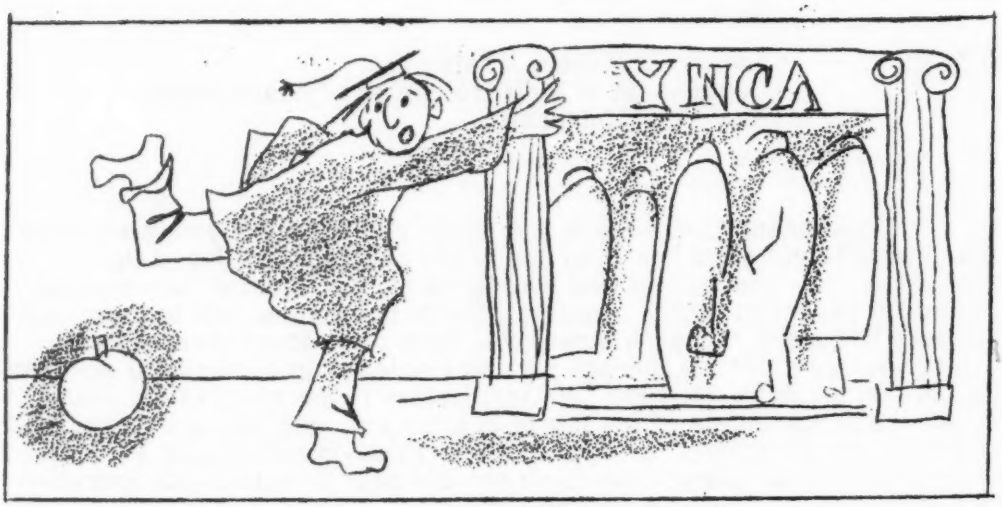
1. Reprinted from the NYU Enginnering Research Review by permission of the author.



was constrained to turn more and more of his teaching duties over to one of his students. He reasoned, quite correctly, that the substitution of a student as teacher in his place would benefit both the student and the student' s students: the former because, in teaching, his own knowledge would be enhanced; and the latter because, in being taught by one near to them in age and interests, they would more eagerly grasp at the scraps of knowledge that came their way. Newton, whose stipend was small, did not spoil this idyllic arrangement by offering pay to his student substitute: a prime example of his sense of values and his restraint. Eventually, when his substitute had proved his ability as a teacher, Newton turned all the classroom work over to him and was thus able to channel all his tremendous powers into the administrative work of the college.

At about this time Newton, whose mind was too active ever to let scientific problems recede from his attention, occasionally mulled over the great discoveries of Kepler on planetary motions and the hypothesis advanced by a number of astronomers, that these motions were governed by an attraction that varied inversely as the square of the distance between planets. One evening of a crowded day in the year 1680, a committee that was scheduled to meet at eleven o' clock, no earlier time being available, was unable to muster a quorum because of the sudden death from exhaustion of one of the older committee members. Every waking moment of Newton' s time was so carefully budgeted that he found himself with nothing to do until the next committee meeting at midnight. So he took a walk—a brief stroll that altered the history of the world.

It was on this excursion into the night air of Cambridge that Newton was struck by a flash of insight which set off a chain of events culminating in his announcement of the law of gravitation to the world in 1686. The season was autumn. Many of the good citizens in the neighborhood of the modest Newton home had apple trees growing in their gardens, and the trees were laden with ripe fruit ready for the picking. Newton chanced to see a particularly succulent apple fall to the ground. His immediate reaction was typical of the human side of this great genius. He climbed over the garden wall, slipped the apple into his pocket, and climbed out again. As soon as he had passed well beyond that particular garden, he removed the apple from his pocket and began munching it. Then came inspiration. Without prelude of conscious thought or logical process of reasoning, there was suddenly formed in his brain the idea that the falling of an apple and the motions of planets in their orbits may be governed by the same universal law. Before he had finished eating the apple and discarded the core, Newton had formulated his hypothesis of the universal law of gravitation. By then it was three minutes before midnight, so he hurried off to the meeting of the Committee to Combat Opium Eating Among Students Without Nobility.

In the following weeks Newton' s thoughts turned again and again to his hypothesis. Rare moments snatched between the adjournment of one committee and the call to order of another were filled with the formulation of plans for testing the hypothesis. Eventually, after several years during which, according to evidence revealed by diligent research, he was able to spend 63 minutes and 28 seconds on his plans. Newton realized that the proof of his hypothesis would take more spare time than might become available during the rest of his life. He had to find accurate measurements of a degree of latitude on the earth' s surface, and he had to invent the calculus.

Finally he concluded that he must find some relief from his collegiate administrative burdens. He knew that it was possible to get the King' s support for a worthy research project of definite aims, provided a guarantee could be made that the project would be concluded in a definite time at a cost exactly equal to the amount stipulated when the project was undertaken. Lacking experience in these matters, he adopted a commendably simple approach and wrote a short letter of 22 words to King Charles, outlining his hypothesis and pointing out its far-reaching implications if it should prove to be correct. It is not known whether the King ever saw this letter, and he may not have, being overwhelmed with problems of state and plans for pending wars. There is no doubt that the letter was forwarded, through channels, to all heads of departments, their assistants, and their assistants' assistants, who might have reason to make comments or recommendations.

Eventually, Newton' s letter and the bulky file of comments it had gathered on its travels reached the office of the secretary of HMPBRD/CINI/SSNBI—His Majesty' s Planning Board for Research and Development, Committee for Investigation of New Ideas, Subcommittee

for Suppression of Non-British Ideas. The secretary immediately recognized its importance and brought it before the subcommittee, which voted to ask Newton to testify before the Committee for Investigation of New Ideas. Some discussion of Newton' s idea—whether it could really be called British in intent—preceded this decision, but the transcript of the discussion, filling several quarto volumes, clearly shows that no real suspicion ever fell upon him.

Newton' s testimony before HMPBRD/CINI is recommended to all young scientists who may wonder how they will comport themselves when their time comes. His college considerately granted him two months' leave without pay while he was before the committee, and the Dean of Research sent him off with a joking admonition not to come back without a fat contract. The committee hearing was open to the public and was well attended, though it has b een suggested that many of the audience had mistaken the hearing room of HMPBRD/CINI for that of HMCEVAUC—His Majesty' s Committee for the Exposure of Vice Among the Upper Classes.

After Newton was sworn to tell the truth and had denied that he was a member of His Majesty' s Loyal Opposition, had ever written any lewd books, had traveled in Russia, or had seduced any milkmaids, he was asked to outline his proposal. In a beautifully simple and crystal-clear ten-minute speech, delivered extemporaneously, Newton explained Kepler' s laws and his own hypothesis, suggested by the chance sight of an apple' s fall. At this point one of the committee members, an imposing fellow, a dynamic man of action, demanded to know if Newton had a means of improving the breed of apples grown in England. Newton began to explain that the apple was not an essential part of the hypothesis, but he was interrupted by a number of committee members, all speaking at once in favor of a project to improve apples. This discussion continued for several weeks, while Newton sat in characteristic dignity waiting until the committee wished to consult him. One day he arrived a few minutes late and found the door locked. He knocked circumspectly, not wishing to disturb the committee' s deliberations. The door was opened

by a guard who told him there was no more room and sent him away. Newton, with his logical way of reasoning, deduced that the committee did not wish to consult him further, and forthwith he returned to his college and his important committee work.

Several months later Newton was surprised to receive a bulky package from HMPBRD/CINI . He opened the package and found it contained a variety of governmental forms, each in quintuplicate. His natural curiosity, the main attribute of the true scientist, provoked him into a careful study of the forms. After some time he concluded that he was being invited to submit a bid for a contract for a research project on the relationship between breed, quality, and rate of fall of apples. The ultimate purpose of the project, he read, was to develop an apple that not only tasted good but also fell so gently that it was not bruised by striking the ground. Now, of course, this was not what Newton had had in mind when he had written his letter to the King. But he was a practical man and he realized that, in carrying out the proposed project, he could very well test his hypothesis as a sort of sideline or by-product. Thus, he could promote the interests of the King and do his little bit for science in the bargain.

Having made his decision, Newton began filling out the forms without further hesitation. One of the questionnaires asked how the funds allotted for the project were to be spent. Newton was somewhat taken aback to read that £ 12,750 6s. 3 d., the surplus remaining in the horticultural development fund for the current fiscal year, had been estimated as the total cost of his project. Methodically, he put down his own stipend first, and after a moment' s thought he added the item: "Other salaries, travel, supplies, and overhead: £ 12,750 0s., 0d."

A true believer in correct administrative procedures, Newton sent the completed forms by special messenger to the Dean of Research, for transmittal through proper channels to HMPBRD/CINI . His adherence to established procedure was rewarded a few days later when the Dean of Research summoned him and outlined a new plan, broader in scope and more sweeping in its conception. The Dean pointed out that not only apples but also cherries, oranges, lemons, and limes fell to the earth, and while they were about it they might as well obtain a real, man-sized government contract to cover all the varieties of fruit that grow about the ground. Newton started to explain the misunderstanding about the apples but he stopped rather than interrupt the Dean, who was outlining a series of conferences he proposed to organize among fruit growers and representatives of various departments of His Majesty' s Government. The Dean' s eyes began to glaze as he talked, and he became unaware that anybody else was in the room. Newton had an important committee meeting at that time, so he quietly went out the door , leaving the Dean of Research in an ecstasy of planning.

The seasons passed, while Newton led a busy, useful life as a member of many committees and chairman of some. One dark winter' s day he was called again to the office of the Dean of Research. The Dean

was beaming; he proudly explained to Newton all about the new contract he had obtained to study the relationship between breed, quality, and rate of fall of all the varieties of fruit that grow above the ground. The project was to be supported by no less than five different branches of His Majesty' s Government plus a syndicate of seven large fruit growers. Newton' s part in the project was to be small but important: he was to direct the Subproject for Apples.

The following weeks were busy ones for Newton. Though relieved from his committee work (a young instructor of Greek, Latin, history, and manual training took his place on the committees), he found himself cast into a morass of administrative problems: forms to be filled out for the governmental departments, for the fruit growers, for the Dean of Research, for the Assistant Dean of Research, and for the financial office of the college; prospective research assistants to be interviewed and hired; office and laboratory space to be wangled from other projects on the campus. The wide abilities of our great genius are fully demonstrated by the way he piloted his subproject during its first formative weeks. He personally filled out 7, 852 forms, often in quintuplicate and sextuplicate; he interviewed 306 milkmaids and hired 110 of them as technical assistants. With his own hands he cleaned out an abandoned dungeon in a nearby castle for use as subproject headquarters; and, turning carpenter in typically versatile fashion, he erected twelve temporary buildings to house his staff. These buildings, used today as classrooms, stand as a monument to Newton' s career.

Soon the subproject was fully implemented, documented, and regimented. Newton was not quite sure what his reconverted milkmaids could do for his hypothesis (he was a lifelong bachelor and hence not well acquainted with the ways of women), but he abhorred the thought of idleness in his staff. So he divided them into six teams, each of which was to measure and tabulate the rate of fall of one variety of apple, using sufficient apples to establish a statistically significant result. All went well except with the winesap team, who discovered a new way of making applejack, and consequently ran short of apples. Newton made a note of their recipe, wisely comprehending long before his fellow scientists the advantages of serendipity, or finding good things while looking for other things.

This period of his life was a happy and profitable one for Newton. From the time he arose in the morning until, exhausted with honest labor, he dropped late at night back into his humble bed of straw, he spent each day filling out payroll forms for his milkmaids, ordering pens and paper, answering the questions of the financial office, and showing distinguished visitors and the Dean of Research around his subproject. Often he discussed the past, present, and future work of his project with representatives of the five governmental departments and seven fruit growers who had been sent to check on his progress. He was frequently invited to give progress reports in person at the central offices of these twelve sponsors. Each week he wrote out a full progress

report which was duplicated and sent by special messenger to 3,388 other projects sponsored by His Majesty' s Government throughout the British Isles.

One of these remarkable documents, in an excellent state of preservation, can be found in the Museum of the Horticultural Society of Western Wales, in the village of Merthyr Tydfil. In typically logical style, the report, bound in a dark red stiff cover bearing the project number, HM2wr3801-g-(293), stamped in gold leaf, opens with a succinct table of contents:

1. Administration
2. Conferences
3. Correspondence
4. Supplies
5. Results of research

The last section, "Results of research," may have been lost during the intervening years, or it may not have been specifically required under the terms of His Majesty' s contracts of that era. At any rate it is not there. But the other sections remain to gladden the hearts of those permitted to read them. Is it too much to hope that this report can be published and distributed among our young scientists in America? Such a precept should accomplish miracles for the morale and spirit of our geniuses.

One day in 1685 Newton' s precise schedule was interrupted through no fault of his own. He had set aside a Tuesday afternoon to receive a committee of vice presidents of the fruit growing syndicate when, much to his horror and Britain' s deep sorrow, the news spread that the whole committee had been destroyed in a three-stagecoach smashup. As once before, Newton found himself with a hiatus. He took a leisurely walk through the luscious vineyards of the Subproject on Grapes, but not, of course, until he had obtained security clearance at the gate. While on this walk there came to him, he knew not how ("Ye thought just burst upon me," he later wrote), a new and revolutionary mathematical approach which, in less time than it takes to tell about it, could be used to solve the problems of attraction in the neighborhood of a large sphere. Newton realized that the solution to this problem provided one of the most exacting tests of his hypothesis; and furthermore, he knew, without need of pen and paper to demonstrate the fact to himself, that the solution fully supported his hypothesis. We can well imagine his elation at this brilliant discovery; but we must not overlook his essential humility, which led him forthwith to kneel and offer thanks to the King for having made the discovery possible.

On his return from this walk, Newton stopped a moment to browse in a bookstore, where he accidentally knocked a book to the floor. With apologies to the proprietor, who seemed in a mood to toss him out upon his ear, Newton retrieved the book and dusted it off. It

was Norwood' s Sea-Mans Practice, dated 1636. Opening the book at random, Newton found it contained the exact information on the length of a latitude degree that he required for the complete test of his hypothesis. Almost instantaneously, one part of his brain performed several lightning calculations and presented the result for the other part to examine; and there it was: the proof complete and irrefutable. Newton glanced at the hourglass in the shopkeeper' s window and with a start remembered that he was due back at the dungeon to sign the milk-maids' time slips as they checked out for the day. He hurried out of the bookshop with the book under his arm, forgetting in his zeal that he had not paid for it.

Thus it was that His Majesty' s Government supported and encouraged Newton during the trying years in which he was putting his hypothesis to the test. Let us not dally with the story of Newton' s efforts to publish his proof, the misunderstanding with the editor of the Horticultural Journal, the rejections by the editors of the Backyard Astronomer and Physics for the Housewife. Suffice it to say that Newton founded his own journal in order to make sure that his proof would be published without invalidating alterations. Regrettably, he named his journal Star and Planet, with the result that he was branded a subversive since Star could mean Red Star and Planet could mean Plan-It. Newton' s subsequent testimony before the Subcommittee for Suppression of Non-British Ideas remains as a convincing demonstration of the great qualities that combined to make him a genius. Eventually he was exonerated, and after enjoying many years of the fame that was due him, reigning one day each year as King of the Apple Festival, Newton died happily.

TITLE: THE EFFECTS OF INGESTION OF CONDITIONED PLANARIA ON THE RESPONSE LEVEL OF NAIVE PLANARIA: II[1]

AUTHORS: Barbara M. Humphries[2] and
Reeva Jacobson[3]
Planaria Research Group
Department of Psychology
University of Michigan

A pilot study (McConnell, Jacobson, and Humphries, 1961) indicated that a possible relationship existed between ingestion of conditioned tigrina and a high number of correct responses by cannibals who ate the conditioned worms. The present study followed a procedure similar to the pilot study, but certain important changes were made. (1) All testing was done blind. (2) All animals used in this study were from a stock started from a single worm, a Dugesia tigrina. Therefore, all the naive worms, conditioned worms, ingesting and ingested worms presumably had the same hereditary background. (3) Most worms were not fed during the testing period.

Study I

Twenty potential cannibal worms were given 30 habituation trials, 15 trials per day on two successive days. The four highest scorers were discarded and the remaining 16 were divided into two groups of 8 each (called "CC" and "XC"), matched for number of responses. The mean number of responses in 30 trials was 3.9 for group CC and 4.1 for group XC; the highest number of responses in each group was 7. During the next twenty days both groups were fed on other worms, receiving a total of five feedings spaced four days apart. Group XC was fed conditioned worms who had reached a criterion of 13/15 responses or better; group CC was fed naive animals which had been kept in semi-darkness for the same period of time as the conditioned worms. Each cannibal was fed 1/4 of a good-sized worm and was fed the same part of the worm at each feeding.

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1. This research was supported in part by a grant from the National Institute of Mental Health.

2. Now in the Department of Philosophy, Harvard University.

3. Now at the Britannica Center for Studies in Learning and Motivation, Palo Alto, California.

Three weeks after the initial habituation trials, groups XC and CC received conditioning trials at the rate of 25 trials per day. The last feeding took place two days before the start of the conditioning. They were offered food again before the second day of conditioning, but most did not eat. There were no differences between worms which refused food this time and those which did not.

Worm Number	Responses in First 25 Trials	Responses in Second 25 Trials
CC 1	4	3
CC 2	4	14
CC 3	6	Not Run
CC 4	7	Not Run
CC 5	6	8
CC 6	5	3
CC 7	4	7
CC 8	11	2
MEAN	5.9	6.2
XC 1	9	7
XC 2	10	4
XC 3	18	17
XC 4	15	5
XC 5	19	2
XC 6	15	5
XC 7	14	4
XC 8	4	7
MEAN	13.0	6.4

TABLE I

After the first 25 trials (a) Most worms refused to eat up to two weeks later; (b) several worms refused to run. Worms XC 1, CC 1, XC 7, and CC 8 had eaten heads; XC 4, CC 5, XC 6 and CC 4 had eaten tails; the remainder ate pieces from the middle of the worm. There is no indication of correlation between part eaten and performance, except that XC 3 and XC 5 had both been eating the left side of the middle portion (as had CC 2 !); such a correlation is not supported by other studies.

Study II

Eight worms were placed in a semi-dark room and received no preliminary testing. Three days later they were divided randomly into two groups; group XC to be fed a conditioned worm, group CC to be fed a naive worm. The worms were fed 24 hours before the first conditioning trials. No attempt was made to feed them during running.

Twenty-five trials per day were given on the first two days of training. In the third set of trials, only 15 trials were given.

Worm Number	Responses in First 25 Trials	Responses in Second 25 Trials	Responses in Third 15 Trials
CC 9	6	3	3
CC 10	1	5	0
CC 11	3	6	1
CC 12	10	15	10
MEAN	5	7.2	3.2
XC 9	13	20	7
XC 10	8	4	3
XC 11	12	12	3
XC 12	8	14	5
MEAN	10.2	12.5	4.5

TABLE II

If the number of responses in the third set is extrapolated for 25 trials, the mean is between 6 and 7. This would make the second and third day of Study II quite similar to the first and second day of Study I. This suggests (a) that the number of feedings received may not be relevant, and (b) that performance may be a function of time delay after feeding, rather than number of trials given .

Study III

Four worms received one feeding (two worms on conditioned worms, two on naive worms) one day before running; they were given 15 conditioning trials 18 hours after feeding.

Mean for conditioned-eaters:	4.5
Mean for naive-eaters:	3.5

Study IV

Sixteen worms were fed about 24 hours before running. Eight ate naive worms; of the two worms eaten by the other eight, one had reached criterion, but had been a low scorer (1/15) in the preceding ten days, the other had been given about 50 trials and had not reached criterion. Although this feeding was done by mistake, the results serve as an interesting control study.

Worm Number	Responses in 25 Trials	Worm Number (Naive eaters)	Responses in 25 Trials
XC 15	6	CC 15	5
XC 16	5	CC 16	2
XC 17	16	CC 17	20
XC 18	3	CC 18	17
XC 19	8	CC 19	20
XC 20	15	CC 20	12
XC 21	9	CC 21	2
XC 22	10	CC 22	10
MEAN	9.0		11.0

TABLE III

If the results of Study I and Study II are combined , in relation to each set of 25 trials, the means are:

	XC	CC
First 25 Trials	11.6	5.4
Second 25 Trials	9.45	6.6

However, if the results are combined in relation to the time delay after the last feeding, the means are:

	XC	CC
Two days after feeding	12.75	6.55
Three days after feeding	6.7	6.1

Further studies are required to determine if, in fact, there is a definite relationship between the time of feeding and the time of testing. This may be responsible for the higher response level of cannibal tigrina who have been fed conditioned tigrina as compared to cannibal tigrina fed naive tigrina, when all the animals originated from the same worm and differ only in learning experience and not in heredity.

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"The body of a worm and the face of a man alike have to be taken as chemical responses." Sherrington. Man on His Nature, 1951.

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"In all natural objects there is some marvel, and if anyone despises the contemplation of lower animals, he must despise himself." Aristotle.

TITLE: THE COEFFICIENT OF LOVE

AUTHOR: Joseph R. Royce, Head
Department of Psychology
University of Alberta

A most shocking realization to the scientist is the discovery that he is also a human being. The fact that such a man is at times irrational, emotional, and frustrated is, to his way of thinking, a humiliating tendency which needs to be overcome.

Take for example the psychologist, the scientific student of behavior, employing the utmost of objective analytical investigation. There' s no doubt that knowledge of psychology enhances analytic thinking as well as introspection, but it is debatable how healthy such a tendency might be if too persistent. And there is at least doubt as to the value of the so-called use of psychology on one' s friends. With the growth of lay interest in psychological and psychoanalytic literature the writer can conceive of the dawn of a new psychic era, which might express itself somewhat as follows.

The newspeak in the psychological world of the future will have gone beyond Huxley, Orwell, and Skinner as obviously too peripheralistic and thereby incapable of getting at the underlying driving mechanisms of behavior. There will clearly be a return to a more scientific eclecticism, involving a coalescence of the psychoanalytic and behavioristic approaches. For example, children possessing too much ibid (youthful id) will not be permitted to play hide and seek for fear they may be emotionally scarred for life. They will live in a specially developed vacuum, a kind of existential Skinner box, until they pass the hide and seek "critical period."

The Huxley soma pill, presently available as alcohol or dope, or in a variety of tranquilizing and energizing pills, will finally have been developed in its ultimate form. Such psychobiological technology will have taken us to the very edge of heaven, for pain will have become extinct, and pleasure will rule the land. There will be no inhibitions, but nobody' s feelings will get hurt—ever! People will not become emotional—they will <u>always</u> be rational. <u>Everything</u> (including sexual relationships) will be <u>mechanized, streamlined, scientific</u>. (For example, telemetering devices will pick up changes in heart rate, blood pressure level, EEG, etc. during every sex act).

Furthermore, in such a utopian scientific era individual desire will obviously not be necessary since applied psychology will have determined what is best for each of us. Even a love affair, which is obviously in a prescientific stage today, will not be left to chance. Love will be dispassionate, consistent, more realistic. A love affair may well be reduced to increased circulation, loss of glycogen, and

fatigue. Subjective emotional states will be discounted as irrelevant and unnecessary. John will not write poetry, become disoriented, or drool at the mouth. Mary will not sigh, roll her eyes, or throw her hips. Once the basic components of love are determined by factor analysis it will be merely a matter of time before it is appropriately reduced to a formula, which will be known as the Coefficient of Love. In fact, we already have a preliminary version of the formula in the recent publication of the mathematical psycho-social engineer Pearson Popenoe. The paper appeared in the last issue of the Very Formidable Journal of Really and Truly Scientific Psychology.

The formula is as follows:

$$\text{I. L.} = \sqrt{\frac{32.99AG}{2^n} + (BP)^2}$$

where I. L. = intensity of love

A = concentration of adrenalin

G = concentration of glycogen

B. P. = blood pressure level

n = number of spouses or/and previous lovers

The scores range from plus one to minus one, with the score of plus one prognosticating a perfect positive relationship, and minus one indicating a perfect negative relationship. It should be noted that the intensity of love varies in direct proportion to blood pressure, adrenalin, and glycogen, and inversely with the number of amorous involvements. Since n is an exponent there is a radical decrease in the value of I. L. as n increases. When n 0 I. L. attains its maximum value. This means, for example, that love intensity is highest when one has never been amorously involved, and that it decreases in value each time there is a love involvement. All other things being equal, the person who has married for the third time is less amorous than he was at the time of his first marriage.

In order to insure objectivity, each man, woman and child will be given a serial number. At the onset of puberty males will be differentiated from females by the Body Litmus Test,* the shade of

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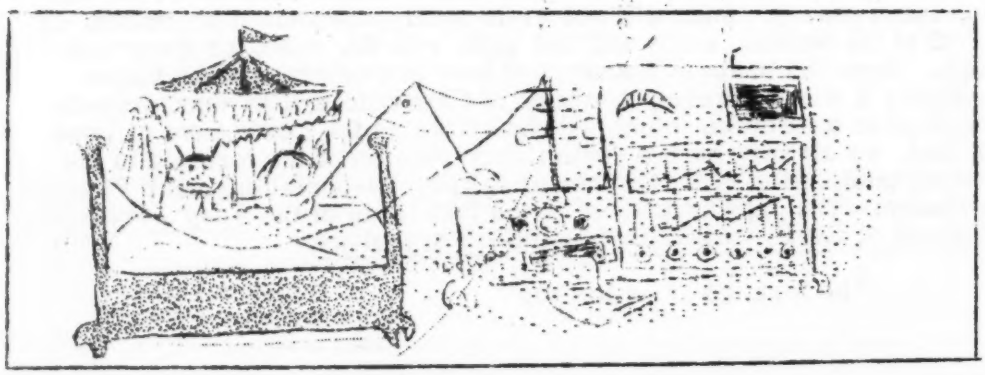
* If they turn blue they are males; if they turn pink they are females. Intermediary colors will be interpreted according to the color pattern which results.



the changed color of the skin indicating the degree of development as well as the sex. Blood tests will be taken regularly and comparisons made with previous samples to insure stability of content. After being appropriately measured, the proper values will be substituted into various formulae for the prediction of various activities.

As an indication of things to come we have only to look at the results which were somewhat hesitantly revealed by the local Institute of Really Human Relations. Admittedly several generations ahead of their time, the authorities pointed out that according to their prediction tables, based on the program outlined above, they could have predicted the following nuptial occasions of our generation: Albert Schweitzer and Gypsy Rose Lee, Mickey Rooney and Dagmar, Clare Booth Luce and Elvis Presley, and Marilyn Monroe and Arthur Miller. **

** Editor's Note: This last prediction deserves special comment. The author of this article indicates that the first version of this paper was written May 9, 1949 at a low point during his graduate student days at The University of Chicago. The prediction regarding Arthur Miller and Marilyn Monroe was actually made at that time. The point to be made is that this degree of accuracy (i.e., one out of four) is as good as most mathematical formulations in the contemporary psychological literature.

TITLE: THE CLASS WAR

AUTHOR: Robert Sommer
Department of Psychology
University of Alberta

Exams take all the fun out of teaching. They turn friends into enemies, make the classroom a battlefield, and set the hostile troops against their leader. The class war described by Marx is insignificant compared with the classroom war. The joy of learning and the joy of teaching become submerged in an atmosphere of cleverness, casuistry, and immorality. Exams create an impassable gulf between instructor and student. No matter how informal, friendly, and relaxed things had been in the past, they can never be quite the same again. The instructor knows that he must sit in judgment on his students. The little girl with the cute legs in the front row may never smile at him again nor will the tall fellow in the heavy sweater come up after class. Exams turn everything upside down in the instructor's mind. The stupid-looking lout who sits in the back row and jokes with his neighbour writes the best examination of all. The students who look interested and come to him afterwards turn in poor papers. With a heavy heart the young instructor gives C's and D's to his favourites and some of the sought after A's and B's to members of the anonymous sea of faces who look completely bored during the class hour. At judgment day both the instructor and his class have been found wanting. No longer is he the genial and easy-going fellow who keeps his class amused during the period (they all use to say it was "the shortest hour on campus"). They are no longer his eager and alert students ready to take down his every word and cherish it until the ink fades and the paper turns yellow. The class war, buried during the honeymoon of the first weeks of the class, now comes into the open. The instructor feels alone, frustrated, and bitter. The students are disillusioned, hostile, and revengeful. They have learned that their instructor is like all the rest, a teacher at heart.

In theory at least, none of this is inevitable. A student who has learned his lessons should be pleased to have an opportunity to given evidence of his achievement. The instructor should look forward to discovering how much of his lessons has been absorbed. Exam time, like the lectures that precede it, should be an occasion for both the student and the teacher to learn. It should be more of a guide for the future than an evaluation of the past.

As things now stand, for instructor and students alike, exam time is composed of dread, panic, and boredom. The order of appearance of these conditions depends on which side of the desk the person sits. A student is initially bored going through the notes, then develops a sense of dread as exam time approaches, and finally panics at the amount of preparation that remains to be done. The instructor dreads the tedium and paperwork of exam time, becomes subsequently bored reading fifty essay answers, and if he does not develop panic at the wide gaps in his students' knowledge, he will certainly be overwhelmed by the prospect of getting the papers assembled, graded, and in the hands of the administr ation before the deadline. While the students are away on holiday for a well-earned rest, the instructor is working his hardest performing tasks that could be done better by a clerk or IBM machine. In my own case about the only way that I can read fifty versions of the same essay is to keep a bottle of scotch handy. I' ve never tried to see how the grades I give early in the evening compare with those I give around midnight but this should make an interesting project some day. On one occasion I reached for the bottle without looking and spilled some of the precious liquid on the papers. I don' t know what the students thought when they received their exams back with the ink blurred and the paper mottled. I never inquired since I feel that in such cases discretion is the better part of valour.

It is one of the miracles of Academia that grades from examinations always fit the normal bell-shaped curve. The supreme tests of a teacher are whether he can get his grades into the proper form and into the registrar' s office before the deadline. His success depends on many factors—if the students write legibly, whether his wife is willing to tear herself away from the television long enough to help him grade the papers, and if he is able to delegate the task to a student assistant. If he has a willing wife or a hard-working student assistant, marking papers is comparatively simple. The only problem is getting the grades to fit the normal distribution of the particular university. The actual midpoint or average of the grades varies from school to school. Here in Alberta the average grade is 62 while some other places it is closer to 70. This doesn' t mean that our students are any worse than students elsewhere, but only that the midpoint of the curve has been shifted downward. With sufficient juggling an instructor is usually able to get the curve of his grades to resemble the typical curve of his university, since some relatively foolproof methods have been developed for ensuring that this happens. For example, the scores can be multiplied by a fraction, or a constant can be added or subtracted, or a general essay question can be included which is graded according to the score of the student on the earlier sections of the exam (if the student did well on the early parts of the exam, his essay question is graded low; if he did poorly on the early sections of the exam, the essay can be graded leniently. It is always assumed that students cannot legitmately question the grade given to an essay answer.) A grading system can be complex, deceitful, irrational, and arbitrary—the only thing it can' t be is transparent. As long as

the students don' t understand it, everything will be fine. The teacher will still be a father figure, the students will remain subservient, and everyone will believe that universities are moral and rational enterprises.

I remember when I once made the grevious error of explaining to the students exactly how I had graded their papers. This may not sound like a startling admission, except that I told them the truth— that I deliberately arranged their grades so that they would approximate a normal curve. This produced expressions of outrage, chagrin, and surprise. Those students who did poorly on the exam were outraged while those who did well were merely surprised. One student came up afterwards and said, "Why don' t you use a straightforward marking system like they use in our science courses. They don' t fool around there, you get one grade and that' s it." I tried to reason with the student but to no avail. It seemed to me that the only difference between my grading system and that used by the other departments was that I made mine explicit. My sin was not that I used a different system, but that I told the students about it. In this sense I undermined the authority of my office. Previously my students believed that exams were constructed in heaven so that good students would pass and poor ones would fail. Now they realized that the only rationality lay in the instructor himself— he can make the grades come out as high or as low as he choses, simply by applying or withholding his red pencil. Being able to exercise arbitrary authority does not in fact increase the absolute authority of the instructor. He too is accountable to higher officials to ensure that his test results come out in a certain way. Thus he becomes an agent of those above him, perhaps a dean or registrar, or of some abstract arithmetic function, rather than the absolute monarch of the classroom with the bright red scepter. Perhaps my confession to the class was tied in with the greater self-consciousness of the social sciences compared with the physical sciences. In any event it seemed that there were many students who desired their faith in the authority of the faculty and the rationality of the grading system to be left undisturbed. The entire affair left me with a sour taste in my mouth. When I returned the exams, I had to face a barrage of questions from the students. When I divested myself of the mantle of absolute authority, I knew that I was opening a Pandora' s box. The questions I received were quite angry too — students who did poorly wanting to know why they

hadn' t done better. This is why some instructors plan the return of examination papers as they would a military campaign. First of all they make sure that a clear path is available for a quick retreat. Under no circumstances do they let themselves get trapped behind a desk, pushed back towards the wall, or maneuvred into a corner. Ideally they want to be near the door the moment the class bell rings, so that a quick and smooth exit can be made. Another method is to give the exams back immediately before Christmas or Easter holidays. Students are too distracted with other things to be concerned with raising their grades by a few points, and if there is anger or hostility to be vented, it will be done in the student' s home rather than in the classroom.

The folklore of Academia contains many accounts of professors who vie with one another for good students. They compete with one another in seeing that good and thoughtful students are assigned to their classes. No less important although less publicized is the quest for fools. Every class has to have someone who will bring up the rear, and it is usually easier to find people who can fill the top slots than to find candidates for the bottom. This is especially true in senior courses when by this time most of the poorer students have failed out. Deans and registrars take a very dim view of the professor who gives only passing grades. They, and a surprisingly large number of students, feel that this professor is being too easy and penalizing students who have more conventional instructors. Each instructor wants to have a few real idiots in his class. While grading exams, I have noted in myself a mounting feeling of apprehension when all the students appear to be doing well. Then I would utter a sign of relief when I discovered someone who was going to fail the examination. I would hope that I would find others too, so that I could give some of the brighter students really high grades. It is easy to buy popularity with high grades, but it takes extraordinary skill or experience to locate those students who can accept failure graciously.

One redeeming feature of the examination system is that the anxiety is dispelled quickly. This has always surprised me and I' m not sure how to explain it. When I first started marking tests, I believed that if I failed a student he would hold this against me. Though this happened in a few cases, it certainly was not very frequent. I can meet a student who received an "F" and he smiles at me as if I had given him an "A" in the course. Can it be that he has forgotten me already or that he has been crushed by the weight of the system and is now completely passive? A more mundane explanation is that he wants me to get the impression that even if he did fail my course, he really is a nice fellow at heart and that I should take this into consideration when he repeats the course next Fall. Perhaps he views me as an insignificant cog in a vast machine with really no part in his having failed the course. Another possibility is that his faith in the morality of the system is

so deep and abiding that he feels any grade he receives is deserved. I don' t feel that any of these explanations is very convincing, but I can' t find a better one, so we will have to let the answer await further research.

Up to this point I' ve discussed primarily the intangible effects of the examination system. Its pernicious influence can be seen in other ways, especially in the state of the students' health. I do not mean only those students who suffer emotional or physical breakdowns during test week. Even among students in good physical and emotional health, there are symptoms of what may be called "exam anxiety". A colleague and I conducted a small survey on this question and we were surprised at the prevalence and intensity of these conditions. Apparently their anxiety starts building up a few days before exams. There is some positive value in this because some students report that they can' t start studying unless they have some anxiety. On the other hand too much anxiety can overwhelm and disable the student.

During exam week some students become ravenous eaters, nibbling throughout the day, and consuming a fantastic amount of coke, coffee, and chocolate. Other students find themselves unable to eat as much as usual, leaving food on their plates, and generally have poor digestion for what they do eat. There are also people who can' t sleep, tossing and turning throughout the night, and then to compound their misery, they have difficulty getting up the next morning. Some students need people during exam time and compulsively seek out company wherever it can be found. Curiously some of these students get irritated by people more easily during exam time and so never stay with people for very long. Like bees drawn to light, they can' t stay away from people but they can' t stay with them very long either. Of course the same thing happens with cigarettes. The student finds he is lighting one cigarette after another, but never gets around to smoking them. Maybe this is what inspired the Madison Avenue copywriter who was responsible for the impertinent question, "Smoking more and enjoying it less?"

Students have difficulty expressing just how they feel at exam time, since they lack a suitable vocabulary. Some of the descriptions include terms like nervous, itchy, edgy, giggly, diggy, and woozy. It is not always easy for an outsider define these words, but the students seem to have no difficulty in understanding them. A fortunate few are able to rise to the occasion, and can concentrate better and think more clearly during exam week. This may be the only time during the year when these students can bring themselves to read the textbook or review their lecture notes. These are undoubtedly people who are at their best in time of fire, flood, or earthquake. It is a sad commentary on university life that exams have to be included in the category of natural disasters.

All right, you may say, exams have to go, but what can be given in their place? One solution that never seems to lack advocates is to replace the lable "examination" with a less offensive term. Exams might be called "exercises", "group discussions", or perhaps a specific term might be coined for each group—instead of exams, medical students might be given "check-ups", business students might be given an "annual audit", art students a "group showing", while music majors could have a "written performance." The justification for this plan is that it is much easier to change the name of something than to change the thing itself.

Nonetheless there may still be a few radicals who want to alter or even abolish the present system of examinations. To these people we can only offer sympathy and pity. They will receive support from no one, least of all the students. Older graduates have forgotten the suffering produced by examinations, recent graduates would like to see others suffer as they did, and the students are so involved in the system and fearful of offending those above them, that they have neither the time nor the inclination to give vent their feelings in any coherent and tangible way. This means that the exam system will continue on indefinitely, or at least until the point is reached when universities become so large that even an IBM 707 can' t score the answer sheets within the time limits set by the Registrar' s Office.

TITLE: CONFIRMATION OR REFUTATION OF LATENT LEARNING IN EARTHWORMS?

AUTHOR: Rodabé P. Bharucha-Reid[1]
Department of Sociology
University of Oregon

I welcome the chance to reply to a criticism of the study "Latent learning in earthworms" (Bharucha-Reid, 1956). The following is taken from a fuller, but informal reply, made to Dr. Arbit when he finished his study, four years ago, on a failure to confirm latent learning in earthworms. The opportunity is welcome because formally and informally Arbit's study of 1957 has been publicized, yet never actually published till recently in the Worm Runner's Digest (1961).

Before entering into any specific criticism of the Arbit study, a general observation is in order. It has always seemed that scientific work whose avowed purpose is to confirm or refute past work by using better controls in the replication, should attempt to keep in mind a similarity in design whenever possible. Confirmation or refutation by a comparison of results is hazardous at best when a number of differences exist in the experimental conditions. This is even more so when the experimenter performing the replication seriously believes that he can compare the actual figures obtained in the results of the two studies (Arbit, 1961). The questions that arise are twofold. (1) Does Arbit's experimental design replicate the earlier experiment of Bharucha-Reid's, but with better controls? And what are the dissimilarities? (2) Does Arbit's study fail to confirm earlier results or in fact to lend some support to the data demonstrating latent learning in earthworms? It is to these two questions that we shall address ourselves.

1. Dissimilarities in design and their significance for comparison of experiments.

1.1. The pre-training maze experience of the experimental group (i.e., the latent learning group) in the two experiments differed. While in Arbit's study the animals were removed every two hours from the maze, totaling ten interruptions, those animals in the Bharucha-Reid study were allowed to explore the maze for massed periods of five hours each. This dissimilarity in pre-training has been pointed out by Jacobson (1962) as well as the following difference to be pointed out.

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1. No longer at Oregon.

1. 2. In the original experiment by Bharucha-Reid an experimental group referred to as the latent learning group, and a reinforced group (with no experience of living in the maze) were run. Arbit in his replication essentially had neither of these groups. The experimental group was given a different schedule of pre-training with no control over their movements (see 1. 5.), and the reinforced group was not included at all. Arbit had a control group with previous training in a stem of the maze. Arbit' s contention (1961) that his control group was more appropriate can hardly hold. The Bharucha-Reid experiment never ran a control group and has never claimed to. In fact this has been a criticism of the study. [2]

For a complete test of conflicting hypotheses, it would seem important to run three groups: (1) experimental, (2) control with handling and experience in a runway, (3) control with no handling or experience outside the living dish. Since this was not done by Arbit it would seem that the other alternative is to examine his own data and see what logical conclusions this would bring us to. Arbit' s hypothesis that the difference in the performance of a latent and reinforced group is due to the difference in handling and habituation would lead to his predicting that the following runs would be obtained.

Group	1 Experimental	2 Control	3 Reinforced
Arbit' s Hypothesis (Mean no. of runs)	27	32	(32 + ?)

From Arbit' s arguments groups 1 and 2 would not differ from each other and group 3 would be inferior. His own data for groups 1 and 2 show that they do in fact differ by a small but consistent difference of five trials (which is 1/6 the total number of trials, a relatively large proportion). It is worth noting that the difference is in the same direction predicted by the Bharucha-Reid study. Arbit has no explanation for this difference between his experimental and control groups even though it is at variance with his hypothesis.

For group 3, had it been run by Arbit, his prediction would have to be that the number of trials would be 32, plus an unknown number for the animal to reach criteria. Our own hypothesis in this case would be that groups 1 and 2 would differ with 1 superior, and 3 might be second best. That is, group 3 would take 32 minus an unknown number of trials to criteria (using Arbit' s own data as basis for the prediction). Are these conjectures on our part with some support from the experimental literature, or would Arbit' s prediction be just as tenable? Our prediction is based on the fact that group 3

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2. Personal communication, Drs. Neal Miller and Edward Tolman. 1956

would not suffer from fatigue, an important variable to consider with this animal, nor from negative reinforcement (prods from the brush). Furthermore, one must take into consideration that the number of trials taken by group 1 has been artifically increased by the handicap of fatigue and negative reinforcement. Experimental cognizance of such variables would actually increase the difference between the latent and reinforced groups.

1.3. The time of the day at which Arbit' s animals and ours were run was different. To quote Arbit himself, "My interest in the time schedule utilised in your study (Bharucha-Reid) is due to the finding of our own research that some of the learning phenomenon reported for the earthworm may be replicated only during certain portions of the animal' s activity cycle."[3] My reply to his inquiry was that our animals were run at a different time of the day, and, as a matter of fact at the more disadvantagous time, according to Arbit' s own study on diurnal cycles and learning in earthworms (1957).[4] It is of some interest that Arbit has not seen fit to use the findings of his own study showing that the time factor is an important variable to be controlled for, when attempting to refute the study on latent learning.

1.4. The study utilised different samples of animals in terms of their ecological background. Arbit himself recognises the importance of this factor (see footnote 3) yet makes no attempt to duplicate the sample available through Turtox. It would seem that a difference in the availability of nutrition in the environment, could make for differences in exploratory drive, and the necessity thereof, to store information necessary for the survival of the organism. The temperature variable which Arbit considers important enough to his own experiments (1957), is completely neglected in this case. An article in preparation now (Bharucha-Reid, 1958) investigates the possibility of ecological differences effecting performance rate. The importance of this variable is also discussed by S. K. Roy (1958).

1.5. Arbit' s attempt to replicate the Bharucha-Reid experiment but with better controls (i.e., using a group of pretrial experience in which the animal lived in the stem of the maze) shows in fact an amazing lack of control. A crucial part of the cognitive mapping would lie in the organism being able to explore the area of the T-maze, the differences in the length of the arms, etc. No count was kept by Arbit of the number of times an animal in group 3 entered each of the arms or three alleys. It is quite possible that the animals

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3. Personal communication, Dr. Jack Arbit, 1957.

4. It is to be noted however that the work of S. K. Roy (1958) indicates no significant difference between day-night activity or per hour activity.

moved back and forth mostly in the stem or mostly in the two arms of the crosspiece. Arbit' s experimental and control groups would have no distinguishing feature and be similar in their pretraining. The small difference in runs obtained by Arbit could have been predicted since there was no difference in the structuring of the map. This is important, because Arbit' s whole argument stands on the sole grounds that although the difference between groups exists as predicted by Bharucha-Reid, the difference is too small to be statistically significant.

1. 6. The dissimilarity in the statistical significance of the results has been mentioned. One must agree that the conservative treatment requires that such a small difference as Arbit obtained, be considered as no difference at all. First of all, we have pointed out above why by necessity this (small difference) is what would happen with the way Arbit ran his experiment. Secondly, Arbit compares statistically his experimental group to Bharucha-Reid' s, and his control group to Bharucha-Reid ' s reinforced group which have little in common, by using the mean runs for each of these groups. While it is valid to reject previous results because of new data and make comparisons between the two, I am not aware of any statistical or scientific rationale which allows legitimate comparison between data based on differing experimental designs. If Arbit is using a rationale that is neither statistical or scientific then it does not seem reasonable to make such a comparison.

One should note that the original statistical significance level of p = . 09 which was close to the significance level has now been raised to . 15 due to " additional data" (Arbit, 1961). What the additional data are is never reported, and while collected over a period of four years it presumably never bothered to correct for any of the dissimilarities mentioned.

Explanation for the difference in results has also been discussed by Jacobson (1962).

2. Confirmation of refutation of latent learning in earthworms?

Despite these radical differences in the two experiments which normally would make one hesitate to make comparisons, let alone refute a study, we are forced to ask the question posed at the start. Does Arbit' s experiment refute or support Bharucha-Reid' s study?

One' s own conclusion in this matter is that for a complete answer it is important to run the three groups already mentioned (1. 2.). Without such data with conflicting statistical results showing a highly significant difference between groups in one case (Bharucha-Reid, 1956) and with a small difference in the same direction but statistically not significant (Arbit, 1961) in the other case, and with

totally different experimental design and a lack of adequate control in Arbit' s study, it is indeed not surprising that the earlier results could not be replicated.

Arbit' s experiment in itself, without making comparisons, is a very meaningful experiment. It has shown that when the quality of exploration by the animal is controlled for the performance rate will vary accordingly. Namely, depending on the topographic aspects of the area covered by the animal, the performance rate should vary from a small difference (statistically not significant) to a large difference (which is statistically significant).

Last of all, Arbit' s quotation of Thorpe (1950) to bolster his experiment and views is very interesting. When Thorpe implied that animals show latent learning, and therefore learning on a relatively high behavioral level, he is not implying that the animal structually has to be on the upper end of the evolutionary scale to do so. But in fact, that this is a complex type of behavior activity, that any animal for whom it is essentially characteristic to explore the environment without the satisfaction of any immediate reward, is capable of demonstrating.

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TITLE: ON THE NATURE OF MATHEMATICAL PROOFS[1]

AUTHOR: Joel Cohen
Department of Mathematics
Harvard University
Cambridge 38, Massachusetts

Bertrand Russell has defined mathematics as the science in which we never know what we are talking about or whether what we are saying is true. Mathematics has been shown to apply widely in many other scientific fields. Hence most other scientists do not know what they are talking about or whether what they are saying is true.

Thus providing a rigorous basis for philosophical insights is one of the main functions of mathematical proofs.

Aristotle was among the first philosophers to study mathematical proofs. He invented the sillygism, a device which, because of its absolute uselessness, has interested countless philosophers and logicians. Briefly, by means of a sillygism, one infers a conclusion from a major and minor premise. In fact, logicians are always coming to conclusions. The miracle of it is that they haven' t got around to stopping yet.

The major premise makes a statement about a class of things; for instance, "Not all major premises are ture." The minor premise says that the thing with which we are concerned is a member of the class, for instance, "The last six words of the first sentence of this paragraph are a major premise." From this we conclude, "It is not always true that not all major premises are true." Such is the overwhelming capacity of logic to inform us of the realities of daily life.

We note, however, that premises are essential for sillygisms. A baleful influence on Aristotelian habits among logicians has been exercised by the recent plethora of signs warning "Keep Off the Premises."

Another function of the mathematical proof is to draw probable inferences from mathematical models of physical systems. For instance, given a sufficient number of physical data, the most desirable mathematical model is probably 36-24-36. De gustibus non est disputandum.

With mathematical proof, scientists have succeeded in relating the hitherto disparate fields of thermodynamics and communication engineering in the discipline of information theory. "Information," as technically defined, is proportionate to surprise; the more surprising a message is, the more information it contains. If someone listening over the telephone heard "Hello" at the beginning of a conversation, he would not be very surprised; but his gain of information would be quite large if

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1. Reprinted from Opus, May, 1961, by permission of the author. For further information on this article, see Worms and Things, this issue.

he were suddenly electrocuted. Great new possibilities in mathematical proof were made available with the development of set theory around the end of the last century and the beginning of this one. A set is any well-defined collection; examples are: the country club set, the table please set, permanent hair-set upset. A theorem in set theory recently discovered by this author is eminently worthy of mention here; the proof will be sketched.

THEOREM: A set whose only element is a set may be isomorphic to a set whose only element is a set whose only elements are a subgroup of the group of elements in the set which is the only element of the set with which it is isomorphic [sick].

This intuitively obvious theorem follows rather deviously from the first isomorphism theorem of group theory. The duty of the logician, however, is to find the shortest logical line between hypothesis and conclusion. In his interest we give the following proof by a familiar method.

Hypothesis: we assume the entire existing body of mathematics.
Step 1: "By inspection" the theorem follows.

The aesthetically appealing simplicity of this method of proof has made many students revere the power of logic. The beauty of this method is exceeded by that of only one other, practiced by Immanuel Kant and first explicated by this author as "proof by assumption." By assuming the desired conclusion in the hypothesis, the proof is somewhat simplified.

Besides "proof by inspection" and "proof by assumption," we have to consider "proof by induction." Induction is so widespread that even the Army does not hesitate to use it. So mechanical is its application that there exists an electronic device cleverly called "induction coil." While inductive technique is simple, its results can be both deep and profound.

The inductive principle is based on a set of five axioms stated around the end of the last century by an Italian musician named Piano. Piano was trying to teach his bambino (Italian for "child") some arithmetic. The first axiom was that zero is a number. Any idiot knows this, which is why Piano was a musician and not a mathematician. Axioms two, three and four were on a similar level of sophistication. For the fifth axiom, we must introduce the idea of a property. The numbers 1, 4, 9, and 16 all have the property of being the square of some natural number. If we call this property F, then we can say, "1 is F, 4 is F," and so on. Now let F be an arbitrary property, for instance, "monotonous," "incomprehensible." Piano' s fifth axiom was: Every number is F if the property F satisfied the two conditions: (1) zero is F; and (2) if any individual is F, then so is its successor. At this point Piano' s bambino wet his diaper.

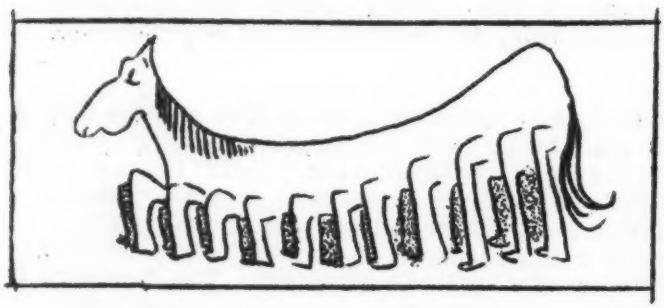
This brings logical systems to mind. A logical system is distinct from a collection of theorems much as a mansion differs from a brickyard, in a logical system each theorem is based upon what has preceded. G. Polya has observed that Euclid' s contribution was not in collecting geometrical facts but in arranging them logically. Had he thrown them together randomly he might have been just an ordinary author of high school texts.

To illustrate the various methods of proof discussed above, we give an extended example of a logical system. (For the first theorem and lemma of this sytem, which I propose to call "the pejorative calculus," I am indebted to Professor Lee M. Sonneborn, Fine Topologist, of the University of Kansas. Dr. Sonneborn is initially known among his students as "L. M. S. F. T." The rest of the system is presented for the first time in this paper.)

The Pejorative Calculus

LEMMA I. All horses are the same color (by induction).

Proof: It is obvious that one horse is the same color. Let us assume the proposition P (k) that k horses are the same color and how this to imply that k + 1horses are the same color. Given the set of k +1 horses, we remove 1 horse; then the remaining k horses are the same color, by hypothesis. We remove another horse and replace the first; the k horses, by hypothesis, are again the same color. We repeat this until by exhaustion the k + 1 sets of k horses each have been shown to be the same color. It follows then that since every horse is the same color as every other horse, P (k) entails P (k +1). But since we have shown P (1) to be true, P is true for all succeeding values of k, i. e., all horses are the same color.



THEOREM I. Every horse has an infinite number of legs (proof by intimidation).

Proof: Horses have an even number of legs. Behind they have two legs, and in front they have fore legs. This makes six legs, which is certainly an odd number of legs for a horse. But the only number that is both odd and even is infinity. Therefore horses have an infinite number of legs. Now to show that this is general, suppose that somewhere there is a horse with a finite number of legs. But that is a horse of another color, and by the lemma that does not exist.

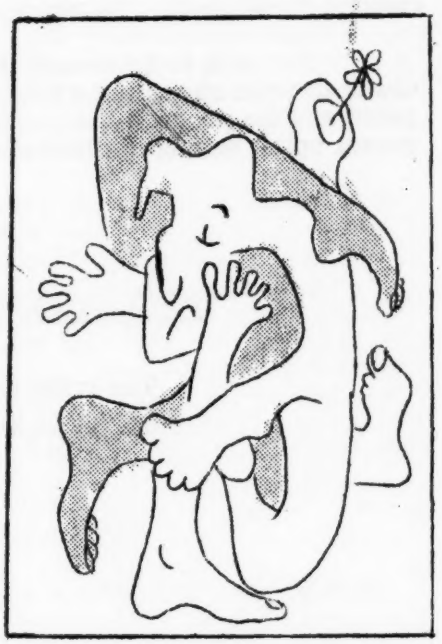
COROLLARY I. Everything is the same color.

Proof: The proof of lemma I does not depend at all on the nature of the object under consideration. The predicate of the antecedent of the universally-quantified conditonal " For all x, if x is a horse, then x is the same color," namely, "is a horse," may be generalized to "is anything" without affecting the validity of the proof; hence, "for all x, if x is anything, x is the same color." (Incidentally, x is the same color even if x isn' t anything, but we do not prove that here.)

COROLLARY II. Everything is white.

Proof: If a sentential formula in x is logically true, then any particular substitution instance of it is a true sentence. In particular then, "for all x if x is an elephant, then x is the same color" is true. Now it is manifestly axiomatic that white elephants exist (for proof by blatant assertion consult Mark Twain, "The Stolen White Elephant"). Therefore all elephants are white. By corollary I everything is white.

THEOREM II. Alexander the Great did not exist and he had an infinite number of limbs.



Proof: We prove this theorem in two parts. First we note the obvious fact that historians always tell the truth (for historians always take a stand, and therefore they cannot lie). Hence we have the historically true sentence, "If Alexander the Great existed, then he rode a black horse Bucephalus." But we know by corollary II everything is white; hence Alexander could not have ridden a black horse. Since the consequent of the conditional is false in order for the whole statement to be true the antecedent must be false. Hence Alexander the Great did not exist.

We also have the historically true statement that Alexander was warned by an oracle that he would meet death if he crossed a certain river. He had two legs; and "forewarned is four-armed." This gives him six limbs, an even number, which is certainly an odd number of limbs for a man. Now the only number that is even and odd is infinity; hence Alexander had an infinite number of limbs. But suppose he had a finite number of limbs. Then it would be possible to put his limbs in a one-to-one correspondence with

the natural numbers, an operation which we shall call "limbing;" and there would exist a last limb, and we should be able to limb it. But only an infinite series approaches a limb it. Hence he had an infinite number of limbs.

We have proved: Alexander the Great did not exist and he had an infinite number of limbs.

It is not to be imagined from this merely compendious account of the nature of mathematical proofs that everything has been proved. Witness the celebrated paradox of Euler' s little liver lemma concerning the four cooler problem. Specifically we cite two unproved examples. The first is the famous Goldbrick conjecture from the theory of numbers, which states that every prime number is expressible as the sum of two even numbers. No counter-example has been found to this seemingly artless assertion, and the search for its proof has occupied mathematicians for centuries.

The second example is a generalization well known, even if only intuitively, to practically the whole uncivilized world. It is Finagle' s famous First Law: "If something can go wrong, it will."

Nor is it to be thought that there are not other types of proofs, which in print shops are recorded on proof sheets. There is the bullet proof and the proof of the pudding. Finally, there is 200 proof, a most potent spirit among mathematicians and people alike.

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Two Theories of Sociological Development

The tribe of Jukes
No doubt had flukes.

The family Edwards
Turned wisely bedwards.

—Mildred Cleaves
Clarksburg, Maryland

TITLE: A RADIATION "BINGE" FOR PROJECT GLOWORM*

AUTHORS: Raymond R. Kauffman and
Margaret L. Clay
Planaria Research Group
Department of Psychology
University of Michigan

It has been demonstrated in rats, mice, rabbits, guinea pigs, dogs, monkeys, and perhaps other species we haven' t heard about, that chronically administered sublethal doses of radiation show a cumulative effect which is measurable but which is considerably less than the damage induced from a single acute dose of like magnitude (1, 3, 4, 6, 7, 8). Some research has also been done on mutational effects of chronic radiation in insects (5). To our knowledge, however, planaria have never been the subjects of such experiments. Our observations on the extreme sensitivity of these organisms to radiation compared to other species of their size and complexity suggested that we might find them to be atypical in their reaction to chronic radiation also.

We have speculated previously (2a) that the flatworm' s extra sensitivity to radiation may arise from his specialization as a regenerating organism. If this is so, and lethality at a low dosage occurs because of destruction of the animal' s ability to regenerate, sublethal doses given at intervals which allow time for the remaining regenerative cells to remultiply might allow tremendous doses of total radiation to accumulate without observable damage to the organism.

To test this hypothesis, we took survivors of a previous study of lethal dose limits (2b) and subjected them periodically to further whole body irradiation in sublethal amounts.** These animals had originally been given doses ranging from zero roentgens (r) to 900 r in 100 r steps. All animals and tail drops from groups receiving 800 and 900 r died within three weeks of irradiation. The remaining groups were irradiated at varying periods thereafter, receiving at each session either no radiation or 400 r of x-ray irradiation under our standard irradiating conditions (250 kvp, 20 ma, HVL 0.875mmCu., tsd 40 cm.). Table I lists accumulated dosage and survival percentages for each group during each of the succeeding periods.

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* This research is being supported by a grant from the Atomic Energy Commission.

** We gratefully acknowledge the cooperation of the University of Michigan School of Public Health, Environmental Health Department, in allowing us access to and technical assistance with their x-ray facilities.

SUMMARY OF DOSE ACCUMULATIONS AND PERCENT ANIMALS SURVIVED AFTER EACH IRRADIATION

(Percentages and time periods for each radiation sub-summary are based on the number of animals receiving that particular irradiation, and the time elapsing since that irradiation.)

IRRADIATION # 1 — 100 r steps from 0 to 900 r

GROUP	A	B	C	D	E	F	G	H	I
ORIGINAL DOSE	0r	100r	200r	300r	400r	500r	600r	700r	800r
% SURVIVED (30 days)	208.3	269.2	183.3	238.5	188.2	151.1	118.7	53.8	0
" (90 days)	241.7	223.1	141.7	238.5	170.5	269.2	156.2	107.7	0

IRRADIATION # 2 — 400 r to each group (105 days post-I, i.e., after first irradiation.)

GROUP	A	B	C	D	E	F	G	H
CUMULATIVE DOSE	400r	500r	600r	700r	800r	900r	1000r	1100r
% SURVIVED (30 days)	85.7	82.2	105.6	90.0	100	105.3	89.5	156.2
" (65 days)	85.7	82.2	105.6	90.0	100	105.3	89.5	206.2

IRRADIATION # 3 — 400r to each group except H_2 (175 days post-I)

GROUP	A	B	C	D	E	F	G	H_1	H_2
CUMULATIVE DOSE	800r	900r	1000r	1100r	1200r	1300r	1400r	1500r	1100r
% SURVIVED (30 days)	93.3	84.6	100	106.7	86.7	100	100	126.7	113.3
" (60 days)	80.0	92.3	92.9	93.3	73.3	93.3	100	140	140

IRRADIATION # 4 — 400r to each group except H_2 (245 days post-I)

GROUP	A	B	C	D	E	F	G	H_1	H_2
CUMULATIVE DOSE	1200r	1300r	1400r	1500r	1600r	1700r	1800r	1900r	1100r
% SURVIVED (30 days)	108.3	91.1	100	100	109.1	92.9	92.9	133.3	114.3
" (60 days)	100	81.8	92.3	100	109.1	78.6	92.9	126.7	119.0

IRRADIATION # 5 — 400r to each group except H_2 (290 days post-I)

CUMULATIVE DOSE	1600r	1700r	1800r	1900r	2000r	2100r	2200r	2300r	1100r
% SURVIVED (30 days)	100	90.0	92.3	100	100	100	100	95.8	104.4
" (60 days)	90.9	90.0	84.1	100	91.7	84.6	100	78.9	104.4

IRRADIATION # 6 — 400r to each group except H_2 (325 days post-I)

CUMULATIVE DOSE	2000r	2100r	2200r	2300r	2400r	2500r	2600r	2700r	1100r
% SURVIVED (30 days)	90.9	100	109	107.1	100	90.9	100	104.1	107.7
" (60 days)	90.0	111.1	90.9	78.6	90.9	81.8	100	108.3	176.9

IRRADIATION # 7 — 400r to each group except H_2 (375 days post-I)

CUMULATIVE DOSE	2400r	2500r	2600r	2700r	2800r	2900r	3000r	3100r	1100r
% SURVIVED (30 days)	100	100	90.9	100	90.9	100	100	112	144.4
" (60 days)	100	(all animals lost)		100	81.8	100	100	120	155.5

IRRADIATION #8 — 400r to each group except H_2 (420 days post-I)

CUMULATIVE DOSE	2800r	-	-	3100r	3200r	3300r	3400r	3500r	1100r
% SURVIVED (30 days)	110	-	-	100	100	100	100	107.1	60.7
" (60 days)	100	-	-	90.9	70	100	100	128.6	50.0

IRRADIATION #9 — (400r to each group except H_2 (460 days post-I)

CUMULATIVE DOSE	3200r	-	-	3500r	3600r	3700r	3800r	3900r	1100r
% SURVIVED (30 days)	91.6	-	-	90	66.6	100	109.1	111.7	100

TABLE I

These results indicate no striking or consistent differences among the groups in survival rates over various doses except the initial dose (already reported in 2b) up to a total cumulative dose of 3900 r, except perhaps for improved survival at the highest dose.

These data suffer, however, from several shortcomings which made us feel it expedient to retest the hypothesis in a second, more carefully designed study. The above experiment was conceived after the original irradiation had been given. Consequently it was necessary to use the species distribution remaining for subsequent irradiation. In group H, the only species surviving was Cura foremanii, a species which drops tails and regenerates more readily than the Dugesia that the remaining groups contained in majorities. This species difference might account for the increased survival rate in the H group.

In addition, different numbers of animals were withdrawn from particular groups at different times for various reasons during the study. Percentages for each irradiation in Table I are based on the number of animals receiving that particular irradiation and are therefore representative. However, we cannot summarize the data over all irradiations and arrive at a total percentage surviving after the 190 day period because of these varying withdrawals which, among other things, would markedly effect overall regeneration rates in the depleted groups. We, therefore ran a second study eliminating species variation and more carefully controlling dose rates and animal population.

Our second experiment used four groups of animals (20 per group), all Dugesia tigrina, carefully matched for size assortment to equalize natural regenerative rates and likelihood of spontaneous tail drops. Irradiation was administered once a week over a 260 day period as follows: All groups were transferred from home bowls to celluloid tubes (20 ml.) and transported to the x-ray facilities for irradiation. They were then returned to the laboratory and their home bowls. Group W received no irradiation. Group X got 100 r of x-irradiation per week, Group Y received 150 r per week, and Group Z was given 200 r per week. The total cumulative doses for the four groups respectively were zero r, 3600 r, 5400 r, and 7200 r—given over a 260 day period. If administered in one single acute dose, total quantities received by groups X, Y, and Z would have killed all worms within three weeks or less, according to previous experiments.

Table II lists survival percentages for each group at 30 day intervals through the 260 day period of weekly irradiation.

Table III summarizes these data, showing initial (30 day) survival and final (260 day) survival percentages for each group.

The differences between the groups are not significant. The trend, if anything, again suggests that a very high quantity of cumulative radiation given in sublethal doses may facilitate rather than inhibit

PERCENTAGE OF COMBINED WORMS AND TAILS SURVIVING CHRONIC WHOLE BODY IRRADIATION GIVEN IN EQUAL DOSES WEEKLY OVER 260 DAY PERIOD

GROUP	W	X	Y	Z
Number in Group	20	20	20	20
Weekly Dose (r's)	0	100	150	200
Total Cumulative Dose (r's)	0	3,600	5,400	7,200
% Survived				
30 days	85	95	90	90
60 days	70	90	80	90
90 days	80	70	65	70
120 days	65	60	55	70
150 days	60	50	55	70
180 days	60	50	50	70
210 days	55	50	45	65
260 days	50	50	45	70

TABLE II

PERCENTAGE OF COMBINED WORMS AND TAILS SURVIVING WEEKLY IRRADIATION AT 30 AND 260 DAYS

GROUP	W	X	Y	Z
Original Dose (r's)	0	100	150	200
Total Cumulative Dose	0	3,600	5,400	7,200
% Survived				
30 days	85	95	90	90
260 days	50	50	45	70

TABLE III

survival and regeneration. The low over-all survival rate for all animals, including the controls which were never irradiated, may well be a function of inuries sustained in the weekly transfer and transport of animals for the irradiating sessions. As a rule, our colonies of animals in the lab tend to increase in size rather than diminish over a like time period when they are not handled except for feeding and cleaning.

Our hypothesis that tremendous doses of irradiation, if administered chronically in sublethal amounts, could be given to planaria without observable damage to the worms appears to be supported by this research. While a single acute dose of less than 1000 roentgens, given under our irradiating conditions, almost inevitably has caused death in our experimental populations within 21 days, we successfully maintained colonies receiving up to 7200 r given chronically in 200 r weekly doses for a period of 260 days, with no terminal indication that they would not have continued to survive indefinitely.

Study I indicates rather clearly that these sublethal doses can run as high as 400 r without noticeably effecting population survival when longer intervals (a month to six weeks) are allowed between irradiations. In fact, though we can draw no trustable conclusions from these data about the "survival-stimulating" effects of chronic, high dose irradiation, all our data are at least suggestive of this. It is not at variance with our earlier hypothesis to speculate that this effect, if real, might involve selective survival of the most radio-resistant regenerative elements in the animals, such that eventually one might demonstrate a considerably elevated tolerance for a single acute dose which had previously been lethal.

In as much as we were measuring total worm populations surviving at each tally, regenerated tail drops were counted as part of the group producing them. What proportion of the total population consisted of original stock as opposed to regenerated tails, we have no way of knowing. By the same token, we have no information on the relative susceptibility to later irradiation of the original animals and the tail regenerates. However, since the tail drops necessarily all originated from animals receiving earlier radiation, we can legitimately consider them as having had this exposure. But the question of whether regenerating tails are more or less susceptible than adult animals to radiation needs further examination. Arguments can be advanced for either case.

Since regenerating tails presumably are in a state of radically increased mitotic activity, and mitotic processes are particularly susceptible to radiation, we might expect regenerating tails to die more readily than adults when subjected to subsequent irradiation. On the other hand, if, by reason of their survival of earlier doses, tail regenerates contain a larger proportion than normal of the radio-resistant elements we postulated above, they might actually tolerate further radiation better than the original stock.

The next experiment that suggests itself involves chronically irradiating at varying sublethal doses varying time intervals up to many times the acute lethal dose, ending with a whopping terminal dose of at least 2000 r —

perhaps more. To differentiate between effects on original stock and regenerates, tail drops should be maintained and counted separately from the "parent" animals as they appear, though they should be subjected to the same later experiences in order that the data can also be treated collectively.

If we' re right, the LD_{50} for the irradiated groups at the time of the final acute dose will be several times higher than for a group which has not received the chronic dosage first. How much higher, and whether original or tail drop stock will do better can be settled empirically. Can we talk anyone into taking on this experiment?

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"To develop a really freindly feeling for a jellyfish or a flatworm takes a lively imagination. And even to tell head from tail in many invertebrates one also needs some information." Buchsbaum, R. and Milne, L. J. The Lower Animals: Living Invertebrates of the World. New York: Doubleday, 1960, p. 13.

TITLE: A NEW TOOL

AUTHOR: Lio Twente
728 Irving Avenue
Syracuse 10, New York

With new barriers in psychiatric research being smashed open every day a new science is slowly revolving. The experiments in sensory deprivation have plowed the way. Everyone has read about subjects being suspended in a tank of water to study how they become depraved when deprived of sensory stimuli. From these experiments in water suspension a new discipline is emerging. This has some precedent in the development of Chinese Brain-washing, but obviously complete suspension in water is more comprehensive. This slowly revolving discipline may properly be termed HYDROANALYSIS. Soon every egg-head will clamor to be hydroanalyzed so that he can break out of his shell and properly mix into things. Deep down in the heart of every egg-head is the desire to be batter and batter. In other words there's no end to what he thinks he can cook up. Naturally it would break him up to do it.

While the classical archaic method of psychoanalysis has shown some failure in dealing with Immature Personalities, hydroanalysis is certain to surmount this difficulty along with other undissolved problems. Egg-heads are a good example since eggs are quite immature forms of life. At the basis of immaturity reactions is the common sense observation that the individual is not yet dry behind the ears. Hydroanalysis will surmount this obvious defect by making the individual wet all over. Properly immersed in the problem the individual should have no difficulty in arriving at a satisfactory solution for it. In fact in being hydroanalyzed the solution becomes so obvious that the individual is unable to escape it.

Should the hydroanalysand attempt to escape this would be interpreted as resistance. The hydroanalyst would be obliged to put his foot down thus reimmersing the hydroanalysand. Now everyone knows that any kind of sand, even analysand, doesn't have much resistance to water, so that minimal activity on the part of the hydroanalyst serves very well. The success of the relationship requires that the hydroanalyst be behind the analysand so that the poor fish can't see when the foot is coming. Otherwise he might be successful in evading the hydroanalyst's probes and scuttle the therapy and the therapist.

With a steadily deepening relationship the two parties are able to dissolve any misunderstanding that may have reflected previous lack of communication. It is obvious that the analyst has to put his foot down deeper and deeper to outwit the increased thrashing about of the resisting hydroanalysand, who may rust as well. This is a consequence of the increased salt content from tears which accumulate over a period

of time. One has to cry in his own tear so to speak. Rusting is a rather unique form of resting and must also be classified as resistance to be safe. One would simply be called a rusting resistor by the hydroanalyst to point out that by thrashing about, crying, and rust-resting he is essentially stagnating the hydroanalysis even though the water keeps moving.

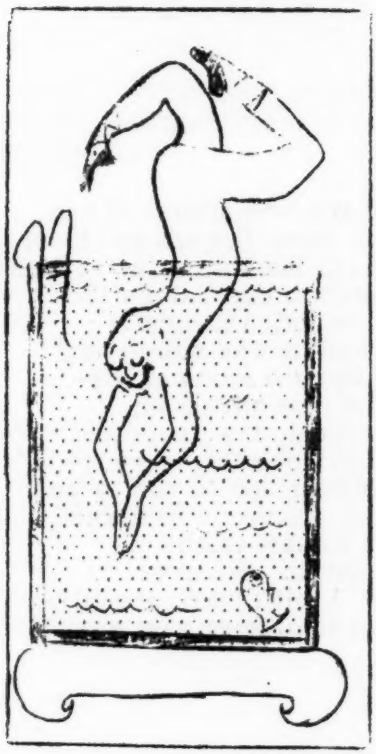
Thus once the hydroanalyst and his hydroanalysand have gone below the surface of the problem and are intimately involved in its solution, the two parties by mutual participation and a little Red Cross technique thrown in for good measure, are able to reach a satisfactory distillation of meaningful material. Some hydroanalysts have an aversion to getting their foot wet, but they find they have to get into it with both feet eventually.

The immature character is able to see and feel the full impact of the hydroanalyst's observation that he, hydroanalysand, is indeed still wet behind the ears. By this stage the hydroanalyst's own ears are also soaked which helps make the plight of the hydroanalysand obvious to both.

With this newly felt knowledge the previously weighted-down, sinking, sad feelings are replaced by a new sense of bouyancy. The hydroanalysand has solved bouy-girl problems so to speak. The disciplined disciple of the discipline emerges from the depths of his hydroanalysis and the analytic tank with a better footing in life, somewhat web-shaped none-the-less. He has learned how to get a better footing in life from the hydroanalyst who put his feet down on the analysand's head, a simple maneuver to keep the subject confined to the analytic tank in turning over the solution to his problems. This experience is so intense as to have almost a religious tone. One gets special insight by the "laying-on of feet" in a way.

The hydroanalysand will be better able to steer his own course. He can be skipper of his own shape with everything ship-shape all the time if he likes. He can literally sail through life. No longer is the hydroanalysand immature and fearful about the common ordinary everyday problems. Walking all over people or going over their heads for example is easy and effortless.

With clear eyes, unfogged face mask, and dry, open ears the newly emerged personality of the hydroanalysand is able to enter into the swim of things. He is able to contribute his share toward keeping society afloat. He has learned how to float a loan for example to pay for the hydroanalysis. He is prepared to face whatever storms may come. After all what could be stormier than to have been fighting submergence in the hydroanalytic tank for five years. A fear of having been born becomes a fear that one won't break out of the bag or tank,whichever



the case may be. To put it another way Birth Anxiety is translated into Tank Trauma, something the hydroanalysand can eventually master.

Yes, modern science in HYDROANALYSIS has opened new port holes to happiness. The import of these port holes into the churning tides of men's minds has yet to reach its crest. 170 million people churning in their tanks would really stir up a huge wave of interest. No telling what the foam would fomit. Since more and more people are building their own pools it is not inconceivable that backyard hydroanalytic technique might become as common as the barbecue pit and burger-basting. I will skip further comparisons of these two pastimes too obvious to mention. But a safe prediction is that HYDROANALYSIS will flood mankind with its contributions.

For the present a NEW TOOL in science, HYDROANALYSIS has precipitated on the horizon. Mankind is launched toward new and exciting discoveries. In going deeply into his problems, at least eight feet deep is required, man has gotten closer to his origins than ever before. Since going back to the uterus is impossible, at present, although the Socialists are trying, this is as close as man can reasonably expect to get.

But if a way to the womb is possible hydroanalysis may find it. As JFK says, man faces all sorts of water problems. Thus the hydroanalyzed man may be best equipped to dip into them. Who knows what scientific answers at the bass of things we'll bag next. HYDROANALYSIS may be the NEW TOOL that will fish out the answers man has been asking himself since his watery birth on Earth.

TITLE: ALICE' S ADVENTURES IN EVOLUTIONLAND: PART II

AUTHOR: Thomas Morrill
1403 Stone Road
Tallahassee, Florida

Alice had born in her an idea of what life was about. It had to do with the explosion of zygote into nerve and bone, fingers and toes; the blood' s Magellanic coursings, always home to the heart, the rhythmic, strong and warm; and the brain expanding medulla to cerebellum to cerebrum. Alice' s chemistry rebelled at the evolution theory' s heartlessness and mindlessness. For her, evolution was simply and wonderfully the most important part of life. After the Monster disappeared, and while being hauled by Alice to the next attraction, I admitted to myself that August of the Immortal Meat, the Evolution Telling Ape, and the Monster in Search of an Environment, were a little hard for me to swallow. Why should Evolutionland be run by and for animals only? And about that big TOE: if evolution is so darned scientific, why The TOE pressue? I am just a humble taster of babyfoods—that is my only claim to fame in science. However, because evolution is science' s ultimate object, I think there should be the greatest question and freedom about it, not something vast and mysterious making us believe.

* * *

A fence of tall, close, 1 x 6' s marched beside the road. I immediately thought of some big construction, a skyscraper going up. But over the roar of machinery we heard a living sound, like a thousand dogs howling at the moon, or cows bawling for lost calves. The sign was reassuring; a funeral could scarcely be taking place at

NATSEC
SEIVE WORKS

The area as big as a football field was filled to the surrounding fence with dozens of house-size contraptions; those in the center were bigger still. Over their belts' and motors' clatter and whine rose the terrific wailing. We recognized, half way up a ladder beside the nearest machine, one freshet of this oceanic misery; a gray creature almost Alice' s height bent forward across the inclined, bouncing front. Our seive operator was a large morning dove. It was trying with fumbling bill and wing tips to stuff whatever was rolling down, through the screen. As it worked it "Boo, hooo, hooo, hooed" and quarts of tears fell from its soft, brown eyes.

"Can I help?" pleaded gentle Alice.

"It' s very sad," groaned the dove superflously, as though to say, ' There' s no help in the world. '

This anthropomorphic lacrymation was too much for me! "You' re separating mineral aggregate aren' t you?" I asked matter-of-factly.

Fortunately the dove was a layman so far as the technical language of straining rocks was concerned. The look she now turned full on us said plainly it wasn' t gravel. "I can' t get these swifts through these holes," she yoweled. I could only ask for more information: "Are your ' swifts' ' chimney' or ' fence' ?" The bird let us know they were ' fence' as about a gallon of water broke loose.

Sure enough blue bellied fence lizards were being tormented down the incline, at the bottom shuffled onto a treadmill belt, and carried to the top again, being constantly agitated so they couldn' t escape. The poor things were quite abraded, most with broken legs or even bloody stumps, all minus tails, which seemed to be going through nicely. The dove was trying to seive out a particularly battered old specimen, poking and mashing as he came down. The more he squealed the more the dove poked and the more she "Boo hoooed."

"For the TOE' s sake, ram me through!" shrieked the reptile.

Our feathered symbol of comity at last controlled her remorse enough to explain that she was selecting a lizard species to populate concrete and steel guard rails on interstate highways. "But you should see my second cousin." (This in a little burst of confidence and an almost defiant wing-pointing to a nearby double seive.) "She' s seiving for more compatible mothers and daughters in a new race of split-level house mice. Ph. D. in racial engineering, completely without emotion—you should talk to Edward."

"Random improvements in any individuals won' t do," explained Edward managing, or "birding," macerated mice down her two seives. "I' m looking for separate accidents in thousands of genes governing whole complexes of traits to co-ordinate in mother and daughter." Perched midway on a ladder in a space between the seives, the racial engineer was poking a pulpy embryo mass with one wing, a mangle of adults with the other. "The odds are pretty high," she blubbered despite her reputation for objectivity. "If I find a mother with the desired complex of mutations, they won' t fit her daughter. If I find the right accidents in a daughter..." I began to get the idea. What was happening to little mice bothered Alice.

Then in amazement we watched the runs from under the seives, the pitiful handful that had gone through was being put on top to run again. "That' s another problem," groaned the harried, i. e.,

feathered, operator, "a given set of mutations has survival value only for a given stage of life. Invariably when we run them even a few minutes later, the selected mutants won' t go through." Indeed, not a single mother or embryo survived the new winnowing. And this time as they bounced down, I noticed, beneath the seive wounds, they were all deformed genetically!

"True," mourned the dove. "Mutations are 100,000 to 1 deliterious. Since we can' t seive for adaptive genes but all are pooled in organisms, the 100,000 bad mutations swamp the 1 good. Each generation organisms that are least unfit, not more fit, survive."

Edward had been explaining her work with increasing apprehension beyond that for the seived organisms. Her ' least unfit survive' was voiced shielded by one wing, looking over it up the corridor between the seives towards the connecting brace. Staring down at us was the sign: "The TOE is watching you." By this time the split-level mice were in pretty bad shape, and our thoroughly miserable friend, pointing off toward the center of the seive yard, declared with great feeling, "You should see the community seiver!" Had the ultimate in evolutionary seivery not intervened, Alice and I would undoubtedly have been caught, perhaps our very lives endangered, in a regular cloudburst of tears.

"You are looking at the latest-type selection," the seiver-in-charge informed us. "We used to seive individuals, but as the ecologist Allee wrote, ' All individuals live in communities.' Most injuries you saw in the lizards and mice arose before they went on the seives, from breaking up communities." In a distant section of the yard we could discern doves laboring with sledge hammers. "Then the individual fragments have to be put through seives with every hole different, so they' ll fit the different organisms as well as physical factors or the community. Also, since the holes mostly correspond to other mutants, well—our seives keep breaking down from genetic accidents. So we figure the ' struggle for existence' is among communities; we' re trying to improve whole communities."

This wasn' t working too well. The community seiver was trying birdfully to fit an enormous ball of organisms, soils, waters, airs, and lights, into one of the enormously complex holes in his enormous seive. He stopped as a moment of dreadful weariness overcame him. "Community seiving is worse than individual," he gasped, looking around nervously for The, or any, TOE. "Communities are part of larger communities. There are ' communities of communities'!"

When we were shown the sledge hammer doves, I wondered if we could be in Dante' s Inferno; having seen the community seiver, I was sure. Out on the road again, Alice said half seriously, "You should have put on the spectacles the Monster gave you," and very seriously, "I' m hungry." Just then a family of regular-size mallard ducks overtook us, and they led us to lunch.

* * *

If the reader has ever been overtaken by mallards, he knows it is a funny sight from the rear. The green helmeted, azure epauletted drake marched in the lead, then his tweedy lady, then seven adolescents with ridiculous little wings almost as they came from the egg only pin-feathered and raised to catch up-drafts, I suppose. The drake was counting cadence, "Chug, two, three, four." When I hailed him, he seemed glad to talk to another drake.

"I' m taking the family to eat now. I' m sure you can have lunch at the cafe ahead." There indeed a hundred feet up the road was a gleaming, glassed-in pavilion. "It' s cooperative," he added oddly with a wry glance at Alice. I was too hungry for the remarks or the look to bother me. The drake and I exchanged salutes, and "falling in," Alice and I waddled to the restaurant.

Entering ahead of us were many people, animals that is, families in a festive mood. The place looked clean, the sign

CAFE

FAMILY STYLE

was in keeping with the happy meal-time occasion. Marveling at our luck, I directed Alice to a small table at the side near the door. The place was full of families. There were: foxes, deer, bear, hedgehogs, agouties, garter snakes, limpkins, greater and lesser kudus, even a family of tapirs, all sitting down together. It was heart warming. Next I noticed the legend, gothic lettered in blue on the wainscoating, completely circling the room above the windows. Not wanting Alice' s interest in the other guests to cause her to miss this classic from The Origin of Species, I read aloud: AS THE INDIVIDUALS OF THE SAME SPECIES COME IN ALL RESPECTS INTO CLOSEST COMPETITION WITH EACH OTHER, THE STRUGGLE WILL GENERALLY BE MOST SEVERE BETWEEN THEM...

It seemed an odd sentiment for a family restaurant, but on second thought I realized that in Evolutionland any sentiment of Darwin' s was appropriate. I had always accepted this ' closer the kin, closer the competition' principle in evolution. Certainly the selection of slight individual variations requires that organisms compete most severely with those most resembling them; brother would have to eliminate brother with the keenest precision. Families could not be permitted to stand in the way of mutations which would advance, or harm, the species. Too, centrifugal, haphazard mutations would tear the family apart; most friction from accidental hereditary changes would be in the gearbox of the family.

It was, however, one thing to accept a principle in the scientific sense of "evolution," thought of as over-and-above life; it was quite another thing to accept such a principle in day-to-day life. Despite the theory, families had evolved. They not only survived in

spite of individual competition; they evolved on the basis of cooperating differences of sex, age, size and strength. "In all respects" members of species do not so much compete with one another as for one another. And here we were watching the most closely related creatures enjoying one another' s company at meal time. Almost since we sat down, Alice had been trying to get through to me something startling about the well-mannered hedgehogs behind her. Suddenly her mood registered with what I was actually seeing: In family after family, the "order" taken by the waiters, lean, hungry bulldogs, was someone in the family. This was the way it was according to the theory!

My daughter can be as lost on occasion as any other nine year old. Here in Evolutionland her need was to know Evolutionland was lost so as not to be lost from herself. Having been almost in tears trying to convince her blind parent of the theory' s latest unreality, she was at last informing me, by loud stage whisper in my ear, "One of the hedgehogs said, ' Pass the baby'." I laughed to cheer Alice up. "Anibal cannibals," she scoffed, sitting back into her happy self.

It was lucky, I thought, that the anibals were normal size, though with the waiters this did not help; normal size cannibull dogs are not "normal size" enough! Swinging round to question the authoritative drake at the table behind me, I saw that his chair at the head of the table was empty. "I want my daddy!" quacked a moulting teenager. It was quite a blow to me personally. Semen banks and artificial insemination are all right for the race, I suppose, but males will never be obsolete to males!

"You do eat just the maimed, diseased and deformed?" I asked hopefully of the shrew sitting opposite us. A prime young antelope went by in the jaws of a dog. "Absolutely not!" shrilled the shrew mother bolting her offspring. "Got to be Grade Choice or we' d get sick."

"But you do ' evolve' ?"

"Evolve, hell!" screamed the little virago. "Got no time to ' ee-volve.' Too busy eating. And being eaten. Gimmetheketchup!"

The ducklings were no longer calling for their daddy. Instead we were enveloped by passionate smackings and savor or roast duck. A bulldog was approaching. I remembered my The Theory Spectacles. How juicy Alice looked! She grabbed the Specs in the nick of time.

Safely outside after a sprint around the dog, we each held pieces of cooked meat in our hands. Alice traded me her hedgehog back for my drake' s leg. When I promised not to use them in any more "cafes, family style," she gave me the spectables too. They would be indispensible at "The Writer' s Colony." Meanwhile, something unusual, especially for Evolutionland, would happen.

* * *

Alice' s love of adventure had not prevented her growing steadily dismayed since we came down the Evolutionhole. She hadn' t completely convinced her self that the Monster was a fake. The ' cafe, serve yourself' in allaying her hunger only made the idea harder to throw off. Certainly we both longed for human companionship; we longed for something else even more.

We started walking in the original direction around the loop expecting merely to see beyond the Cafe. Our decision to go beyond was made the instant we saw, paralleling the road in banks easy of access, a rushing mountain stream. How pure the gladsome innocence to wash clean our innocence in its own!

Alice lept down the bank, stepped --one--two--three----four, onto a big stream-cradled stone, knelt, cupped her hands. I followed, and we sat on the polished boulder surrounded by water. Alders, poplars, maples, horse chestnuts, pallisade on pallisade, outreached from the tranced earth banks. The sound was of water on water and on stones; the air was of leaf and spray; the light was in green leaves glowing on crystal water dappling, and streaming from the center of the sky. A bird flew low up the current, landing on the rock beside us. I think it was an oozle, a wren-like songster of the high Western mountains. He slipped into the water, first with feet and head, then completely, feeding on larvae encased under the stones. Up he bobbed, and his song was the stream' s,stone' s, air' s,tree' s, sun' s, bird' s, and his own; "Ec-sta-cy."

"Ahem."

It had shoes. It had suit. It had tie. It was a real human being! I had never met him personally, but I knew he was real from his pictures and name being in the newspaper. Not only that, he was the very man I desperately wanted to talk to. From my childhood, Darwinism had been "the" theory of evolution; no other theory was possible; everyone said so. Evolution was a fact because it was so interwoven with Darwinism; Darwinism was the scientific theory of evolution because it was so intertwined with the fact of evolution. Who was I, a mere human scientist, to develop another theory in opposition to that of all other scientists? Anymore than I would assert, by myself, that what was believed as "the theory" wasn' t a theory at all. Now in my Edenbasket of fact-theory, in the forbidden center where fact and faith strands could be unraveled, a knowledge worm was gnawing.

Our "ahemer" was the leading expert on Darwinism in the world, Ph.D., LL.D., F.R.S. and G.K.W. "Sir Bellicose Bunkley!" I spluttered at the top of the bank. "I' m so glad you' re here." In a daze of awe and perspiration I thrust my warm paw into Bunkley' s cool hand. Having declared again how glad I was to see him, I laid before Sir Bellicose for godfathering the naked babe of my life' s crisis:

"I' ve traveled a lot in Evolutionland, and—do genes really change only from such things as cosmic rays? You yourself wrote that mutations are ' 100,000 to 1' harmful. Doesn' t Darwinism have a normal explanation? About that Weismann dichotomy: whether we think of "germ" and "soma," or "DNA" and "protein," don' t you think there' s some orderly mechanism for bringing them together to produce the most important changes in organisms? Maybe by RNA? And couldn' t that ' natural selection seive' be patched up a bit?"

This rant surprised me as much as it must the reader. That events of the morning had given me pause, I well know; that they had quite undermined me I was unaware until I heard the speech. While Alice beamed on her crazed parent, Bunkley ...

The reader may recall that Sir Bellicose has quite a pained expression to begin with, as though he had colic all his life. (An irreverent college chum, an English major in my biology class, used to call him "Sir Bellyache.") Now craggy brows and doleful chops unfolded to comprehend an outward cause of pain. He spoke with cultured and dignified ferocity.

"Surely, my man, you can' t be serious. You talk like a layperson. Natural selection is a mechanism to select one organism in a million; likewise it is a theory understood by a chosen handful of eminent scientists. It takes years, my boy, and what-I-believe-you-Americans-call-' guts.' Darwin discovered nature' s plan, he is the actual father of evolution. All difficulties can be overcome. Go back to the laboratory, my boy. Go back to your Origin!"

Her old self again, Alice was pleased to take her father' s side against the expert. "What is ' natural selection' ? Exactly what is this ' natural selector' ?"

The biologist' s stomach surely pained him almost more than he could bear. One did have to admire the Britisher' s guts, "grits" - I-believe-you-British-call-it. He smiled icily like gas pains, "Perhaps you' ve heard of ' environment' ."

"The trouble," insisted Alice, "is you never explain that." Emboldened, I wanted to say that having gone back to my Origin' very recently, I had been amazed that nowhere was "natural selection" and "nature" defined except contradictorialy.

"That bird singing," Alice shouted. "Listen! The struggle isn' t for 'survival.' It' s for living, for evolving."

"No! No! No! The bird' s singing for no joy of life." (Scowl fisted in black brows framed by white chops atremble.) "He' s marking his territory like a dog wetting a tree. He' s calling his mate to copulate with him." The bird sang and Sir Bellicose made a hurt sound deep inside. Stooping to the road, he whirled as though swinging

a scythe; down the bank clipping the tender green sprays, over the smiling water—stone, singer, and song went down in the boil. The biologist disappeared; Alice ran downstream.

She stood where the stream dropped into empty darkness, sobbing with the imprisoned water. I marveled at modern man' s murder of his innocence, the exorcism of his fatal demon, hope; "Moby Dick" over and over. The sobs ended as the water, receding, its dirge fell away, borne to us from the chasm only on a wavering thread of air. And in the deepest depth we heard the bird singing, "Ec-sta-cy," "Ec-sta-cy," again and again, "Ec-sta-cy."

* * *

Beyond the chasm, which was at the end of the loop, the first attraction towards home was a mound a hundred feet in the air. Animals were cavorting—"A monkey island!" breathed awed Alice as though it was a cathedral covered with saints. A very "evolutionary" monkey island we were to find it, after considerable translation, and help from The Theory Spectacles. Approaching we saw that this island wasn' t the usual baker' s yeasty nightmare of concrete bosses and caves, frosted about with barrels, see-saws and trapeses, rising to an old dead tree. No, the monkeys lived on a three-storied pyramid like a Mexican Maya temple. Going through the gate, we literally "ran into" an old friend.

It was the big, white, World-Worried Rabbit coming out, the one who led us to Evolutionland in the beginning. He hugged to his vest a sheaf of papers which he almost dropped when we bumped, and continued to almost drop in his haste to apologise and to be gone.

"I' m sorry. It all seems so sorry. Been getting help for a speech I' m to deliver, if I' m allowed, before the World Congress on Radiation, Cancer, and Evolution. Made a mistake back in 1927, have the answer here..." (Head down, fumbling in his papers.) "Must hurry. Late. Sorry."

We felt sorry too; he seemed such a nice rabbit through his Theory Spectacles. As he sped off he finally dropped one of the pages he was carrying. Alice ran after him calling, "Rabbit, hey old man, you' ve lost part of your speech." The rabbit man didn' t stop, and Alice returned with the paper. On it were two bits of doggerel. The first was:

"Penny for your sanity,
Protons in your pants—
Hay foot, straw foot,
Everybody dance."

This was crossed out with the notation, "frivolous." Carefully written below in the large, round hand of a child was:

"Boggle, boggle little ray,
In among the D-N-A;
In the cells so dark and deep,
You our genes we give to keep.

Boggle, boggle little ray,
Do not blow us quite away."

As I said before, the monkey island was a stepped pyramid like a Mayan temple. Having come in the gate, we were on the lowest level surrounded by a hundred-or-so monkeys. They were the common spider monkeys of South America, all arms, legs and tail except small body and head. The middle level was forty to fifty feet higher, separated from our level by a steep slope and joined by built-in stairways. This level had less room, and fewer but larger monkeys. On the top, up another staired slope, were just three very large monkeys, in Olympian calm, actually sitting under a small temple with Doric columns. The monkeys all had writing implements, and were writing, in keeping with the "Writer's Colony" sign on the fence.

The big three on top were copying tablets leaned facing them against the temple pillars, writing on similar tablets beside them on the benches. Evidentally they wrote with metal styluses, and their tablets were of stone, for when completed, these were passed down to the middle level with some effort. On arrival of a master's tablet, writers on the second level left their writing, gathered around, read, and chattered eagerly. Then they copied the stone on similar, but plastic, tablets.

When the plastic tablets reached the ground level, the budding and lesser authors swarmed about excitedly. They copied—with pencil and paper, in quadruplicate with carbons—but not so much as the writers on the other levels. In fact, several "naturalists" making notes on Alice and me did not go over to the tablets the whole time we were there.

We watched one dreamer dash off some verse, then as young writers will, instantly broadcast copies of his masterpieces. The poem was read by the poet's fellows, who gave a few words of advice and encouragement before resuming their own furious scribblings. Nor did the sonnet stay on the ground level. We saw it carried by a young courier up the stairs to the middle area where it was read by one of the larger, more serious scribblers there. He, after writing something in the margin, returned it to the courier who took it the rest of the way to the stack of manuscripts in front of a big monkey on top.

That sage, forehead very high for a monkey's, sat in imperturbable concentration, reading the samples from below. Several times he conversed genially with another sage about a particular passage. Completing the pile, he took what was evidently an ordinary pencil from

behind his ear, wrote on ordinary paper a number of comments, which the couriers delivered to the other levels. Meanwhile, the master struck the attitude of Rodin' s "Thinker." When he wrote again, he did so for a time contemplating the Colony rather than the tablet leaned up beside him.

So the whole Colony was a unit. It had its Shakespeares, Goethes, Tolstoys, who were its students as well as its teachers. The masters produced the Colony' s comprehensive, incisive classics; these were read on the frontier where they reacted with life "in the raw," which reactions the masters took into account in future writings. The Colony was sustained by the most amazing equivalence from center to periphery. To the eye, the top masters were just the size of the writers on the group level; space was precisely compensated for by size. The fewness of the master' s writings was exactly compensated for by their scope and permanence. The masters' equipoised, vortex location permitted their superior view and decision, at the same time subordinated them to the Colony. They were shielded; at the same time they were central to the Colony' s refined influences. I was reminded strongly of a cell organism, although contrary to Darwinian theory the intelligence flowed to, as well as from, the genes. Thinking I might not be seeing in "the proper and hopful light," I put on the spectacles.

The change was what the theory predicted. Now the top monkeys had traded their inspired nobility for a most dreary rectitude. So help me! they sat in a row, the left-hand master holding hands over ears, middle master, hands over mouth, third master covering his eyes. They were writing with styluses held in their tails, and as I watched, the speak-no-evil monk mechanically finished a tablet and kicked it down the stairs.

Another change in the masters I hesitate to mention. For one thing, I do not know if the celebrated personages in question had come to resemble the monkeys, or the monkeys, the personages. That is, I do not know if these celebraties were present before I put on the spectacles, when the masters were noble and far seeing, and I failed to recognize them, or only after. In either case, the reader is sure to be offended by the identification of his heroes with a monkey pantheon. Merely as a scientist reporting facts as they occurred, may I observe that through the spectacles the middle master resembled Old Charles himself; the master on the left resembled Sir Bellicose Bunkley; the other, a living paleontologist.

Gone was the Colony' s equilibrium of size, weight, number, height, distance, centrality and peripherality. DNA masters no longer learned from and through their students, recording the tenor of the whole. I was writing these observations on a blank page a monkey dropped when Alice, who had been having fun with a baby monkey, wanted to see through the spectacles. She gazed in wonder at the Olympians, then, "Boy! that hear-no-evil monkey sure ' flipped' ! "

I took the glasses back, too late. The big three were mechanically writing with their tails, the see-no-evil monkey glancing between furtive fingers at the tablet he was copying. Suddenly he stopped writing, gaping straight overhead. Several seconds of dumbstruck immobility were followed by a burst of scrawling. I watched ten minutes more without observing another such seizure. I did observe three times a master sneak a look at his neighbor' s tablet. This cribbing among themselves I believe the geneticists call by the rather apocalyptic term, "crossing over." However, it produced no original lines; the masters' inspiration came solely and pristinely by rare "bolt from the blue." What a contrast the carryings-on below!

Around us all was lewd glancings and folded notes slipped between snickering groups. Nearby a hairy grandfather was exhibiting to a band of wastrels his latest pornography. There was an explosion of guffaws and the crumpled page flew high in the air landing at my feet. "What is it?" wondered sweet Alice, expecting the spectacles to provide some further delight. I flattened the paper and read it. Warily I removed the spectacles, wrapped them in the paper, put them in my shirt pocket. I thought I saw the joke, not only of what was written on the paper, but of all the Evolutionland follies so far:

"Honey," I answered sadly, "You aren' t old enough to understand now. And when you' re grown, it probably won' t be funny to you."

(To be continued)

THE WORM' S - EYE POINT OF VIEW

FROM: Jane Farley, Ph. D.
Director, Psychological Research
Parmly Foundation for Auditory Research
Chicago 26, Illinois

Your admirable journal came to our attention today through a friend on the editorial staff of the AMA publication "Today' s Health"--of all things (you had better do a security check; there' s a leak in your organization somewhere).

For sheer joi de vivre, it rivals the New Yorker, and--to a toiler among the less tidy moils of human research--it also jostles Harper' s Bazaar out of position, with its enviably sleek and elegant designs. As if that were not enough (and manifestly, it is), the W. R. D. is one of the few comparatively inviolate sanctuaries remaining to the non-conformist (though by this time next month, at present publicity rates, who can say?)

For all of the above reasons, we hope you will put aside petty species-snobbery, and add us to your mailing list.

FROM: A. baron van Boecop
Head of the Research Department
Stichting Schoolpsychologische Dienst van de Gemeente Utrecht
Wilhelminapark 27 a, Utrecht, Holland

With much interest I read something about the work of the Planaria Research Group. You should oblige me very much by sending me the published issues of the Worm Runner' s Digest.

I shall forward you the expenses with pleasure. Thanking you for your kindness in advance...

FROM: William M. Smith, Research Scientist
The George Washington University
Washington 6, D. C.

I am not a Worm Runner but a social psychologist. However, I thought you might be interested to know that in Antarctica, where I have conducted some work over the past few years, a most unusual kind of worm has been discovered. The importance of this worm cannot be underestimated for it has found its way into the songs and legends of those who live for certain periods of time in this far away place and it constitutes a very real threat to human life.

The first reference I found to the existence of worms (which have been tentatively names Planarias Antarcticas) in this region of the world, was in the songs that are sung on various social occasions when rather unstructured ceremonies take place and a drink, peculiar to the social groups in this region, is served from bottles and cans. Although I cannot report the music as I do not know musical notation, the verse will, I feel, serve as an illustration:

In the shadow of the pole
Where it's 99 below
And the penguins go a dancing o'er the plain,
In the shadow of the pole
I will clasp her to my soul
I'll be with her when
The Ice Worms nest again.

Reference is also made to the ice worm in legend. Although only a few people have ever seen the ice worm, and lived to report it, you will understand the reason when I tell you this is because of their color and very low body temperature. The ice worm is snow-white in color and with the exception of its eyes which are a bright blue, it does not reflect light differentially from that of the snow surface over which it travels at a speed much greater than the fastest man can run. It is, therefore, almost impossible to see the ice worm except for the eyes.

As for their threat to human life, I must report that their body temperature approximates that of the mean annual temperature of the area in which they are found. Although this varies from one portion of the Antarctic to the other, it is not unusual for them to have body temperatures as low as -80° F. or more. Because of their agility and their tendency to be attracted by warm bodies, their danger to man rests with the observation that once they have made contact, the hapless victim will freeze to death in about two-three minutes (this figure is still in question as no precise measures have been taken as yet).

FROM: Cecelia Simpkins
Central Michigan University
Mount Pleasant, Michigan

During Easter Vacation I came down to the University of Michigan and got your experiment "the effects of regeneration upon retention of a conditioned response in the planarian."

Four of us students, from Central Michigan University, decided to do your experiment as a project in our freshman biology course. We had quite a problem getting things set up and started, so we weren't able to finish the complete experiment. We did manage to condition the worms though, and believe me when we got that far we were ready to consider it a major scientific breakthrough.

Our main problem was finding a suitable material for the troughs in which to shock the worms. We weren't able to have one of opaque plastic, so we made a substitute out of asbestos sheeting which looked like clear plastic. I set a worm in to test the trough and an hour later the worm was dead. I changed the water and put in another worm and an hour later it died. It was from this that we concluded that the asbestos gave off some chemical in the water that was lethal to the worm. We tried five other kinds of troughs made from saran wrap, some other kind of plastic sheeting, hollowed out styrofoam coated with wax, and intravenous tubes from the hospital cut in half--boiled and unboiled. By this time we had poisoned seven worms and I was seriously considering changing the experiment from "Conditioning Flatworms" to "How Many Different Ways To Poison a Flatworm." We finally ended up with a very satisfactory trough made from things around the lab and from the hardware store. It was made of styrofoam, plastic tubing,

clay, wax, and two copper washers.

I have become quite attached to my little cross-eyed beasties, and equally so to my brine shrimp which are very comical and very ferocious looking under the microscope with their one orange eye. I wouldn' t have the heart to throw them back into the mill pond. So I guess I' ll try and do some other experiments with them during the summer.

Dr. McConnell, all four of us fully appreciate now what you said in your article in the first DIGEST about it being "no easy thing to undertake a project using an animal you haven' t learned to live with." We truly believe now in the First Law of Experimental Science.

Doing this experiment has been of great value. We knew that if we followed your directions carefully, we should have about the same results that you had, but the thing that was a lesson to us was working out all the side problems that came up while doing the experiment. Many of the problems were very frustrating, but in the end they all seemed to work out and it has been lots of fun and very rewarding.

FROM: Prince John Loewenstein, Ph.D., F.R.A.I.
5240 Kent Street
Halifax, Nova Scotia

I am interested in your Journal and shall be much obliged if you will forward it to me, including the back numbers for 1961. I am particularly interested in the articles covering your research on flatworms, espe cially the one published at the beginning of this year demonstrating how learning may be inherited by worms which regenerate from halves. I am anxious to know the results of further experiments which might have been carried out since.

Please let me have your bill, including postage, and I shall send you my cheque on receipt.

I shall also be much obliged if you will include a list of publications of your department, as well as a list of the papers by Pavlov.

---(The DIGEST will continue to be sent free to all interested scientists. --Ed.)

FROM: Helen P. Klein
Rt. 4, Box 338B
Norman, Oklahoma

It has been a refreshing experience for me to become acquainted with your August, 1961, issue of the DIGEST. As an embryonic graduate student in experimental psychology I feel that it would be very elucidative for me to have your journal to refer to in times of stress. Therefore, I am requesting that my name be added to your mailing list. In exchange, I would like to make you a charter member of the board of directors of THE JOURNAL OF NEGATIVE RESULTS which we will soon be publishing. I should like to mention, in passing, that this past summer I was engaged in an extensive research project involving the

retention of learning in dogs which had been subjected to eleven minute circulatory arrest. After exhaustive and extensive research, it was concluded conclusively that regardless of how well trained a dog is he cannot withstand eleven minute circulatory arrest.

FROM: Martha Matesich
509 Granville Street
Newark, Chio

This is a long overdue note of thanks to you for sending me the DIGEST. I've been receiving it for almost a year now and have enjoyed it extremely.

For the past two years I have worked on planaria for my school science project. The first year I worked merely with regeneration in planaria. Last year I attempted to duplicate your experiment in training planaria to respond to the light-shock stimulus. Unfortunately, I was not successful here. However, I did make your name known at the science fair!

This past summer I attended a science institute where I worked on a research project on the ectoparasites of damselflies and dragonflies. I plan to continue work on this project this school year.

Planaria have not lost my interest, though. I still have 150 healthy specimens in the basement! I wish I had the time to return to them, for I'd like to try work with mazes. Maybe I'll be able to sometime later.

I hope to continue to receive your publication of the DIGEST. I really think a great deal of it and the flatworm it represents.

FROM: John Paget
The Travellers
25 Avenue des Champs
Elysees, Paris, France

Although not a professional controversialist like Arthur Koestler, you also know how to hand embattled prejudice a bloody nose; so you may be able to borrow a few arrows from the enclosed collection. We had an English humorist before the war who used to quote strong stuff of this nature and underneath he used to write, "Cor stone the crows, that's telling 'em." He was called Nathaniel Gubbins and wrote in the Daily Express, or rather the Sunday Express, in the thirties.

A certain Francis Haines, a colleague of yours from California, interested in spotted horses, and introducted to me by the Ashmolean Museum at Oxford, writes:

"The first spotted horse must have been a mutant some twenty thousand or more years ago, what seems remarkable to me

is that the spotted pattern has persisted for so long."

The spotted hide of my immaculate white baby poodle (or rather, my wife's) gives me grounds to doubt the above statement. Besides white x black = spotted poodle.

However, it looks to me as if an isolated herd of spotted horses under suitable climatic and geographic conditions would rapidly reconstitute the original variant or something very like it; for on Exmoor only the wild indigenous type survives however many outcrosses have been introducted into the herds. Under challenging conditions the original spotted variant of the white horse ought theoretically to recur; or is this pure scientific superstition?

As a matter of fact, I did not order the complete series of your DIGEST not knowing how to pay for it in dollars, but was delighted to receive it.

Go on making scientists write verse. I have a project for confining all diplomatic correspondence to verse. The Secretary of State could once more find time to read his dispatches and fools would be effectively eliminated from the service.

FROM: H. David Hammond
Assistant Professor of Botany
Howard University
Washington 1, D. C.

Thank you very much for the DIGESTS received about a month ago. I have finished reading them (although I got lost in the contribution from the Japanese correspondent--I think it is too near supper time for me not to be distracted by all that talk of meat powder. The School's 10-minute-before-end-of-class bell just rang and I found myself salivating (really!)).

I enjoyed every issue and was especially pleased to note the outline of Dr. Lindegren's book. I hope that it will be forthcoming ere long. Your mention of reprints is also noted. My wife did some work with Schneirla nearly 15 years ago and maintains an interest in animal behavior and would also be interested to have these articles.

The December issue of Scientific American had (I thought) a fascinating grouping of articles and notes. First there was the note on the Planaria work & RNA by Corning and John, then the note on messenger RNA, and then the article by Hyden on nerve cells. I was so impressed that I read all three to my Evolution class.

If Hyden is right, is memory like the formation of adaptive enzymes in microorganisms? Does this explain facilitation? Can we create memories synthetically by artificial messenger RNA? Forgetting would seem to be a homeostatic mechanism.

FROM: Dr. Yasushi Fujita
134 Minamisawa-Gakuenmachi
Kurume-machi, Tokyo, Japan

I am much obliged for your "DIGEST" Vol. III-No. 1 which I have just received, and also for your kind letter dated 9 March, and the separate cover enclosing two copies of your respected works and the lovey DIGEST NN. 1 and 2, which were safely delivered months ago.

I was, of course, eager to reply for your kind letter, but, at that time, my type-writer--with which I had enjoyed the collection of copies of some interested literatures, among them the Draparnauld' s happened to be included--was out of order, and was under overhauling. This forced me to wait the next chance, the arrival of the separate cover.

This next chance, however, visited me in the middle of my busiest time in school--at the beginning of the new school year in Japan--and the adjustment of our curriculum, the formation of the time table to arrange our 30 teachers of so and so lessons to 19 classes (each class having 35 hours of lessons in a week), was one of my most painstaking tasks. Then, the problem of permutation and combination to distribute 30 teachers into 19 x 35 arrays, occupied all of my brain activity, leaving least space to "think in English" to write to you. Here, I must regret my lack of courtesy to learn English enough, to communicate with you at once, in case of need, whenever it may be, notwithstanding whether I am occupied in some other Japanese works or not.

(If I were to live in English speaking country, I might be idle in writing the letters in Japanese. Some day, I want to have a chance to prove this hypothesis).

Now, after reading your recent DIGEST, in the "Worms-eye Point of View" column of which, together with the letters from excellent scholars around the world, mine was cited (I must be grateful for so much of your appreciation of it), I am writing this.

As for Draparnauld' s, I understand his old and fundamental problem as such "To which piece, whether to the right side piece or to the left side one, does the soul of the worm go, when it' s cut in half longitudinally?"

I sincerely wish to expect you to perform the experiment of retention of a conditioned response by the worms cut longitudinally, into left side piece and the right one, besides your splendid previous works done by cutting Ss into anterior and posterior halves.

Your transverse cut experiments clearly showed that the cutting of the longitudinal nerve chords has no effect upon the ability of the retention of a CR (conditioned response) by the worm. --However, let me add--"So long as the lateral commisure or the transversal nerve connections are remained intact."

The longitudinal cut experiment may be available to prove or check the roll of the lateral commisure, in the process of the retention mechanism. May I name this the DFM--Draparnauld- Fujita (excuse me)-McConnell--Experiment!

Whatever the nomenclature it may be, I hope you again, to manage this experiment with the skill of your laboratory. (Then, really "The worms are now truly international!")

"How does the tail retain anything? And when the tail section regenerates a new brain, is the CR built into the cerebral ganglia?" was the closing sentence of your first report (J. Comp. Physiol. Psychol., Vol 52).

"The tail piece also retains the intact lateral commisure" may be one of possible answers, to your first problem. And, to prove this hypothesis, the DFM-Experiment was suggested.

Allow me to discuss a little further, and to suggest one more experiment to answer your second one. To solve your second problem, I have no doubt in it, that the chemical substance hypothesis such as you have suggested in your recent DIGEST, Vol. III, pp. 41-45, is highly effective. Nevertheless, I have another idea, here.

Whether the regenerated half from the conditioned half really has, or has not, the "built-in" mechanism to respond to the CS (conditioning stimulus), is not yet solved, I suppose. In other words, when the tail piece of one conditioned worm allowed to regenerate, the anterior half of the regenerated worm is entirely naive to the conditioning stimulus, or has some thing "inborn" ability to the CR, is not yet clear.

To solve this, I want to suggest the following re-regeneration test.

To make the things simple, I assure here the fact as such, "When a half piece of a conditioned worm recovered the full size, the regenerated half is naive, and the retention mechanism remains only in the old conditioned half."

If so, we can re-cut the regenerated worm into the naive and the conditioned halves, and can easily suppose that, the worm regenerated from the naive half is entirely naive, while the worm regenerated from the conditioned half retains the CR.

Next, we may suppose, the conditioned half can build some kind of the CR-retaining mechanism, in the regenerated part, while it is allowed to regenerate. Then, when we re-cut as above, the worms recovered from both pieces, equally retain the CR.

To answer the question, "Which of the above two hypothesis is the fact?", only the comparison of the retention test, between the worms regenerated from the conditioned half and from the naive (?) half is needed, and in the world, only your laboratory deserves to perform this experiment, I think.

I shall be happy, if I can hear from you, that you have interested in the two (the DFM, and the re-regenerate) experiment any way, and still more pleased, if I can hear the results of them some day. But, the most important thing I want to tell you now, is my hope, that this letter may not bother you too much.

If I can hear, your design of experiment now going in your laboratory has not bothered, it may delight me most.

P.S. Miss (perhaps) Anna Chranowa recut her worms about 18 times successively, in 5 months, from April till August-September, 1937. Wilhelm Roux' Archiv fur Entwicklungsmechanik der Organismen. Bd. 139, pp. 65-77. "Wiederholte Regeneration bei Planarien." Anna Chranowa, Sofia.

---(Dr. Fujita's letters are among the most delightful, charming and least bothersome we receive. For the record, we have never run the suggested "Draparnauld-Fujita-McConnell" study since we have always been more interested in the retention of learning in those sections of the worm that have no brain than in those sections which might well retain half-a-brain. Our later studies with cannibals suggest rather clearly that the transverse nerve connections are not responsible for the retention of the memory, however. The D-F-M study would be an interesting one, though, and we would encourage our readers to undertake it if interested. The second study, which Fujita calls the re-regeneration test, was actually performed by Reeva Jacobson, Donald Maynard and myself a couple of years ago; Reeva is now repeating it using maze experience rather than classical conditioning procedures. --Ed.)

- - - -

The Wall Street Journal (of all publications) ran this piece of verse several years ago:

Why reeks the goat
On yonder hill
Who seems to dote
On chlorophyll?

Well, here's my answer--scientifically correct, by the way:

The chloro-phylled buck
Is rendered smelly
By his reproduc-
Tive tract, not his belly.

—Mrs. Mildred Cleaves
Clarksburg, Maryland